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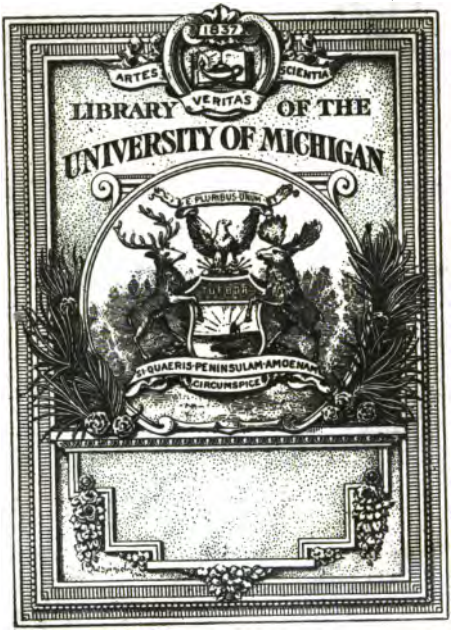
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THE MONTHLY

MICROSCOPICAL JOURNAL:

34808

TRANSACTIONS

OF THE

ROYAL MICROSCOPICAL SOCIETY,

AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

HENRY LAWSON, M.D., F.R.M.S.,

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VOLUME VI.

LONDON:

ROBERT HARDWICKE, 192, PICCADILLY, W.

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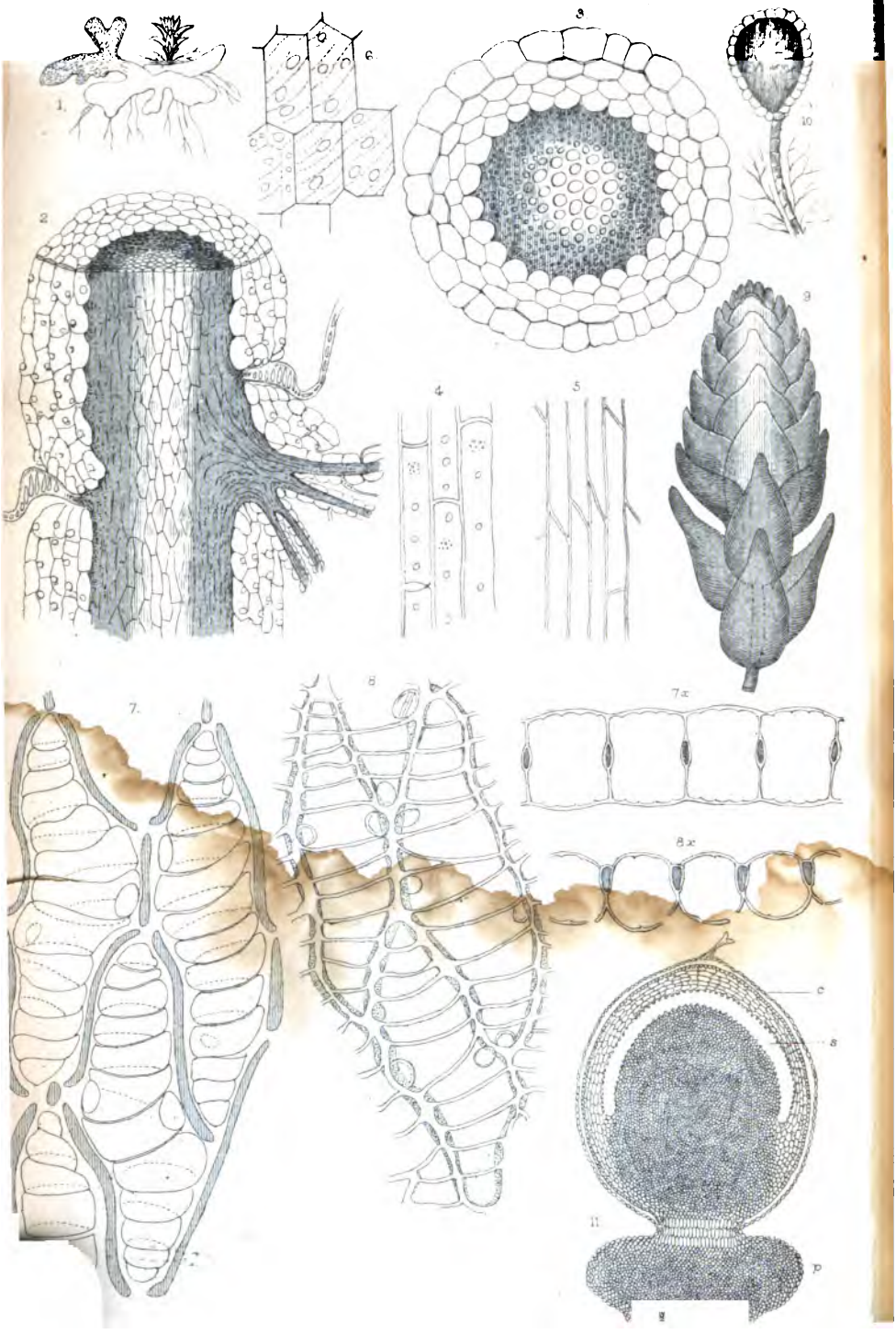
LONDON: PRINTED BY WILLIAM CLOWES AND SONS, STAMFORD STREET AND CHARING CROSS.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THE

MONTHLY MICROSCOPICAL JOURNAL.

JULY 1, 1871.

I.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

(*Read before the* Royal Microscopical Society, *June* 7, 1871.)

Part I.

For a considerable time the plants known as bog mosses have attracted attention, not only by the masses in which they are found growing, but by their peculiarity of structure, and the difficulty of finding characters by which to establish the species, for the varieties are endless, and such a common facies is impressed on the whole group that Linnæus regarded them all as one species, which he named *Sphagnum palustre*. In our own time, however, the careful use of the microscope has revealed to us their wonderful organization, and enabled us to establish many species.

Up to the publication of Professor Schimper's magnificent treatise,* the bog mosses had been universally associated with the other mosses in one class, but in that work they are placed apart, and

EXPLANATION OF PLATE XC.

Fig. 1.—Prothallium with young plant.
„ 2.—*Sphagnum cymbifolium*. Vertical section of stem passing also through two leaves, and the base of a branch fascicle.
„ 3.—Ditto. Transverse section. These show the pith, the woody cylinder, and the four layers of bark cells.
„ 4.—Cells of pith.
„ 5.—Ditto of wood.
„ 6.—Ditto of bark.
„ 7.—Cells of a branch leaf of *S. cymbifolium*.
„ 7*x*.—Transverse section of a leaf of *S. squarrosum*.
„ 8.—Cells of a branch leaf of *S. acutifolium*, seen from the back.
„ 8*x*.—Transverse section of same.
„ 9.—Male flower catkin of *S. cymbifolium*.
„ 10.—Ripe antheridium with paraphyses.
„ 11.—Vertical section of a capsule still enclosed in the calyptra. *c*, calyptra; *s*, cavity of sporangium; *p*, pedicel of capsule enveloped by the vaginula.

(Fig. 1 from a specimen lent by Mr. Howse, the rest from Schimper's work.)

* 'Entwickelungs-geschichte der Torfmoose,' 1858.

hold equal rank with Mosses and Hepaticæ; the three classes, Bryinæ, or frondose mosses, Sphagninæ, or bog mosses, and Hepaticinæ, or liverworts (the Laubmoose, Torfmoose, and Lebermoose of the Germans), thus forming one great muscal alliance.

As it may be of interest to know something of the histology of the Sphagnums, and except the late Mr. Wilson's admirable sketch of the family in 'Bryologia Brit.' we have little in our own language bearing on the subject, I purpose on this occasion to point out the chief characters which distinguish them from mosses, and at a future time, with your permission, give an account of all the British species, with illustrative figures of their structure.

Commencing with the spore, we find that on germination it does not produce the much-branched confervoid prothallium of mosses, but if growing on the wet peat, a lobed foliaceous production results, exactly like one of the frondose Hepaticæ, as Anthoceros, and from this the young plants grow. This was first observed by Hofmeister in 1854. If germination occur in water, the prothallium is a fine filament, the lower end of which forms roots and the upper enlarges into a nodule from which is developed the young plant. As soon as branches form on the plants, in both cases the prothallium and roots wither away. Next, the stem, which in mosses consists of uniform texture throughout, is in Sphagnums much more differentiated, for we observe—1st, a medulla, or central pith, of long cylindric cells by which a current of sap is constantly passing to the growing apex; 2nd, a woody cylinder of firmer prosenchymatous cells; and 3rd, an outer, or bark-layer of one to four strata of thin walled cells, always larger than the rest, and which in *S. cymbifolium* alone contain spiral threads and foramina. The branches also are quite peculiar, and differ altogether in arrangement from those of mosses. In the young state they are crowded into a capitulum at the top of the stem, and as the internodes elongate they become separated, and are then seen to be in lateral fascicles of three to ten, the bundle originating at one side of a leaf base, every fourth leaf in the spiral giving rise to a branch fascicle.

Of these branches, one part spread out horizontally and then arch down, the rest being attenuated, pendulous, and pressed back to the stem, and by these water is conducted to the very apex; indeed, Professor Schimper compares their action to a hydraulic pump, for a tuft of plants placed dry in a flask of water immediately carry on the fluid by the lower bark cells, and empty the flask by a discharge of drops from the down-bent capitulum, but if some of the branches and bark be removed, no passage of fluid takes place beyond the injured part.

Of the sponge-like retention of moisture by Sphagnums, we have often unpleasant experience when stepping on the green turf-like surface of its beds, only to plunge deep into their interior, and

emerge soaked with the water concealed therein. They can also absorb atmospheric moisture and transmit it downward, thus taking up again at night what they had lost during the day; and by this perpetual interchange the stagnant pools in which they flourish never become putrid. The bark of the branches consists only of a single layer of cells in communication with the innermost layer of the stem bark; but these are of two forms, for besides the ordinary kind, there are others of a flask shape, larger, and with a circular orifice, one of which always falls at a leaf-insertion.

The leaves of bog mosses are very peculiar, and are well known as an elegant microscopic object; those of the stem are more remote from each other than in mosses, and in all the species two complete spirals contain five leaves (phyllotaxy $\frac{2}{5}$); their form is ovate or tongue-shaped, with the base frequently more or less auricled. Those of the branches are much narrower, and not only vary on the two forms of branch, but on different parts of each. If we look at a Sphagnum leaf in fluid with a sufficient power, the first thing that strikes us is the beautiful sigmoid form of the areolation, or cellular network; and next, the presence of a delicate fibril forming spirals or rings on the inner wall of each cell, and by which the thin membrane is kept expanded, while perforating the membrane are distinct apertures or pores through which it is common enough to find infusoria have passed, which may be seen sporting about in the cell cavity; and thirdly, that these large prosenchymatous cells are always void of chlorophyl, and hence want the lively green colour so noticeable in true mosses.

This, however, is not all. A more careful examination will show that between the walls of these hyaline cells are placed extremely narrow parenchymatous cells, which do contain chlorophyl. Moldenhawer first detected these in 1812; yet Carl Müller, in his 'Synopsis Muscorum,' terms them intercellular ducts; in no case is there any approach to the formation of a nerve or midrib. According to the colour of the chlorophyl in these parenchym cells, is the colour of the Sphagnum tuft, only seen, indeed, in the living or moist state, but presenting endless shades of rosy red, purple, vinous red, bluish green, olivaceous, apple-green, and straw colour. When dry or dead, the hyaline cells lose their transparency, and all the species become more or less a dirty white. The spiral threads are nothing but thickenings on the inner wall of the cell membrane, such as occur frequently in the tissues of phænogamous plants; in one species only are they altogether absent, *viz.* the American *S. macrophyllum*, which Professor Lindberg separates as the genus Isocladus.

The male flowers of Sphagnums differ also from those of mosses, and in their arrangement, and the form of their antheridia, resemble those of Hepaticæ; they are grouped in catkins at the tips of lateral

branches each of the imbricated perigonial leaves enclosing a single globose antheridium on a slender pedicel, which is inserted at the side of the leaf base. Paraphyses surround them, but instead of being simple, as in mosses, they are very long, much branched, and of cobweb-like tenuity.

The perigynium of the female flower-sheath terminates one of the short lateral branchlets at the side of the capitulum, and is of a deep-green colour, with large sheathing leaves; the archegonia do not differ from those of mosses.

After impregnation the fruit receptacle enlarges and extends itself into a pseudopodium, on which the capsule is sessile; the vaginula was thought to be absent from these plants, and hence Bridel formed for them a section "*Musci evaginulati.*" It is, however, very distinct, being the disk-shaped enlargement at the apex of the receptacle in which the expanded bulbous rudiment of the pedicel of the capsule is completely buried.

The calyptra, or outer cell-layer of the archegonium, has not the definite form common to mosses, but is a very thin colourless membrane closely investing the capsule, by the enlargement of which it is torn irregularly into shreds, the lower portion being left attached to the vaginula.

The capsule up to maturity remains in the perichætium, but after that the receptacle elongates and carries it upward on this pseudopodium, which does not correspond to the pedicel of a moss, that being always found above the vaginula; still, a pedicel does exist in Sphagnum, but it is bulb-shaped, and enveloped by the vaginula. The capsule itself is very uniform in all the species, being almost spherical, the lid only slightly convex, without any beak or point, and we never find any trace of a peristome. The sporangium, or spore sac, does not attain the development found in mosses, for it appears like a hollow hemisphere in the interior of the fruit, its outer wall being coherent with the inner cell-layer of the capsule, its inner wall with the columella lying beneath it.

The spores somewhat resemble those of Lycopods in being of two kinds—macrospores produced by fours in a mother cell and tetrahedral in form, and microspores which are more spherical, and not half the size.

With respect to the function of these plants in the formation of peat, I cannot do better than quote Professor Schimper's words. He says:—"Unless there were bog mosses, many a bare mountain ridge, many a high valley of the temperate zone, and large tracts of the northern plains, would present an uniform watery flat, instead of a covering of flowering plants or shady woods. For just as the Sphagna suck up the atmospheric moisture, and convey it to the earth, do they also contribute to it by pumping up to the surface of the tufts formed by them the standing water which was their cradle,

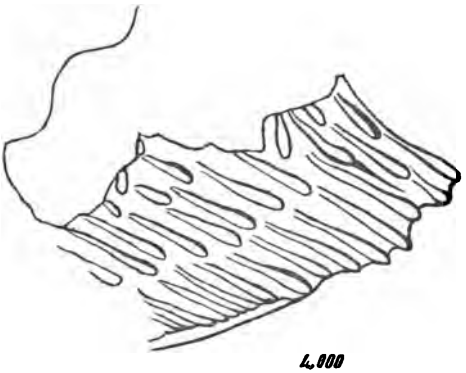
diminish it by promoting evaporation, and finally, also by their own detritus, and by that of the numerous other bog plants to which they serve as a support, remove it entirely, and thus bring about their own destruction. Then, as soon as the plant detritus formed in this manner has elevated itself above the surface water, it is familiar to us by the name of turf, becomes material for fuel, and all Sphagnum vegetation ceases."

Little noticed, then, as these plants may be, they perform functions in the economy of nature which cannot be overlooked; and it is for the purpose of inviting more general observation of their structure that I bring forward these remarks. Some seventeen British species are known, all of which I hope to illustrate; and in carrying out this object, I shall feel thankful for good specimens of any of our species, but especially those occurring in the north of Scotland, for the wild moorlands of Northern Europe have so far proved to be the most prolific in new species.

II.—*Structure of Podura Scales.*

By F. H. Wenham, Vice-President R.M.S.

Mr. McIntire has kindly presented me with a number of slides; two of them are superb specimens of the test Podura, *L. curvicollis*, with markings remarkably dark and distinct. In one of the best scales there is a singular case of fracture, evidently caused by the slipping of the cover, which has dragged the specimen asunder near one-third from the upper end, which is separated to a short distance. Projecting from the largest portion is a fragment, partly torn away, as shown by the accompanying cut, which is an outline sketch taken by the camera lucida, 4000 diameters. On the right-hand extremity the longitudinal tear has taken place close to a rib, or marking, which is nearly isolated. The transverse tear at the bottom leaves most of the ends of the ribs exposed and projecting, as the membrane has torn away from behind them. The extremities of ribs 4 and 5 (counted from the right) are particularly plain and prominent. I am willing to submit this specimen to the inspection of anyone doubting the fidelity of the tracing.



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How it can be maintained or admitted that these waved or constricted ribs which give rise to the "note of exclamation" markings are "illusory," I am at a loss to imagine. The question can only be determined by fragmentary pieces; but in the *Poduræ*, from the toughness and absence of brittleness in the scales, these are very difficult to obtain, and it is seldom that a fortunate accident occurs in the way shown. It was by fragments that the structure of the *diatoms* became known, wherein the silicious nodules could be traced down to a single atom, and their form is not now a question of dispute.

Mr. Hennah, and others, have demonstrated how a number of different patterns, not the least indicative of real structure, may be produced by transparent bodies of *regular* form, such as glass rods and bosses, &c. Much more incomprehensible would these appearances be if the transparent ribbings were twisted, lobated, or in the peculiar form of a Podura marking. The patterns would be innumerable with oblique light, and the whole area might be made to

consist apparently of "illusory beads," and the belief now appears to be, with the most unbiassed observers, that they are such.

The *Degeeria domestica*, or speckled Podura, when shown opaquely * under a $\frac{1}{12}$th or upwards, is a specially beautiful object. The scales are apparently much thicker than in other species, and the ribbings or !!! markings are of a reddish-brown colour—not beaded, but slightly constricted at regular intervals, like the short antennæ of some insects, and in the deep intercostal spaces there are numerous thin septæ, or transverse bars, very fine and distinct, of a greyish tint. Both these and the slightly "varicose" spaces on the ribs may be displayed in the form of beads, by dodging the illumination. Where practicable, some form of opaque illumination should always be employed for verifying the structure of these objects, for we are in this case quite free from the errors of diffraction, which more or less accompany objects seen by transmitted light, and cause an indistinctness of outline.

* The only plan at present known to me of illuminating these and other dry test-objects is the one that I recently described, and consists in mounting them, not on the cover, but on the slide itself. When the light is totally reflected from the upper surface of this at the part where the object is in contact, total reflexion will not occur, but the light will pass into the object, and in many cases—as with some diatoms and the Podura—illuminate it with sufficient brilliancy on a black field to be plainly seen with the highest powers and eye-pieces.

There are several methods of securing this total reflexion, some of which are described in my paper "On a Method of Illuminating Opaque Objects under the Highest Powers of the Microscope," read before the Microscopical Society on March 26th, 1856, and published in the 'Transactions.' One of them consists of a solid glass parabola with a flat top, upon which the slides were placed, with an intervening film of highly refractive medium; this was made at the time, and ever since has been in use, mostly for viewing aquatic animalcules and living diatoms, &c., for which it is especially convenient. The parabola is first lowered so as to bring its top below the level of the brass table, and the object in a drop of water is covered with thin glass. The parabola is then screwed up till the object is secured. The rotifers are beautifully shown this way; but I have found that for objects on slides its use is not so convenient as the small truncated or nearly hemispherical lens and the ordinary parabola, as this arrangement affords greater facility for traversing the object, as there is sufficient play for the lens for this purpose in the hollow top.

As some have complained of a difficulty in using this, I repeat the directions. The little lens is first patched on under the slide with a minute drop of oil of cloves or turpentine where the Podura scales or other objects lay thickest. The slide is then put on the stage with the lens in the top of the parabola. The next thing is particularly to obtain parallel light. A large bull's-eye condenser should be employed with its flat side towards the lamp: hold a sheet of paper over the plane mirror, and see that the light falls in the centre, and is of the same area as the bull's-eye or about fills the mirror; then go through the ordinary adjustments, and if the field is not quite black raise the stop of the parabola: look at the objects with a low power, those only that are brilliantly luminous are *on* the slide. Select a proper one, and if it does not appear nearly in the centre of under lens, shift this beneath it. One cause of failure may be the absence of objects detached from the cover on the slide; but in English mounted objects, either of diatoms or scales, I have in most cases found a few straggling specimens that have left the cover. Possibly the figure of some of the paraboloids may have degenerated since I supplied the original steel template near twenty years ago, which must have been worn out before now; but this I will see to.

III.—*On some New Parasites.* By T. Graham Ponton, F.Z.S., Honorary Secretary Bristol Microscopical Society.

Plate XCI.

Some time ago I described a new parasite from the tiger, in the pages of this Journal. I now propose to present to its readers four parasites, which I believe to be hitherto undescribed, or new.

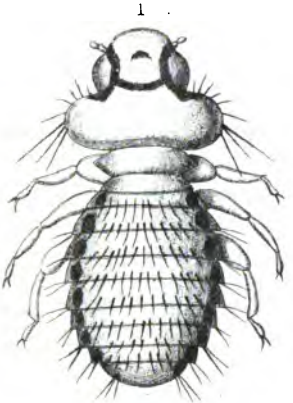
Menopon ptilorhynchi (mihi) (Plate XCI., Fig. 1).—Colour bright fulvous. Head obtusely subtriangular; clypeus rotundate, vertex rounded, base concave. Two broad irregular chestnut markings extend from the insertion of the antennæ to the eyes, which are connected at that point by a semilunar chestnut line, a chestnut spot in the centre of the clypeus; prothorax elliptical; metathorax transverse; abdomen ovate, hairy; all the segments, except the last, have a chestnut spot; legs long, tarsi clavate. Length 2·115 mil. Habitat, *Ptilorhynchus holosericeus.*

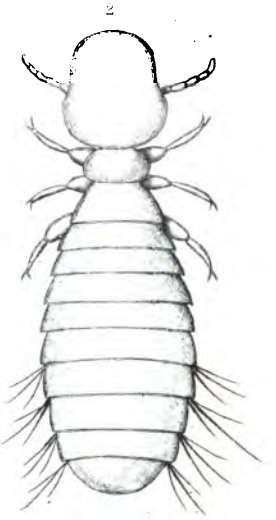
Nirmus Nitzschii (mihi) (Plate XCI., Fig. 2).—This species is probably the same as that mentioned in Giebel's list of the Halle Collection, without either name or description. Supposing this to be so, and that it is undescribed and unnamed, I have ventured to call it *Nitzschii*, after the indefatigable student of these peculiar insects, C. L. Nitzsch, and beg to give the following description of it:—Colour pale yellowish-white. Head panduriform, clypeus rounded, antennæ rather long, second joint longest. Prothorax not so wide as the head; metathorax oblong, trapeziform. Abdomen lanceolate, a long fascicule of hair between each of the four last segments. Legs somewhat clavate. Length 2·538 mil. Habitat, *Ptilorhynchus holosericeus.*

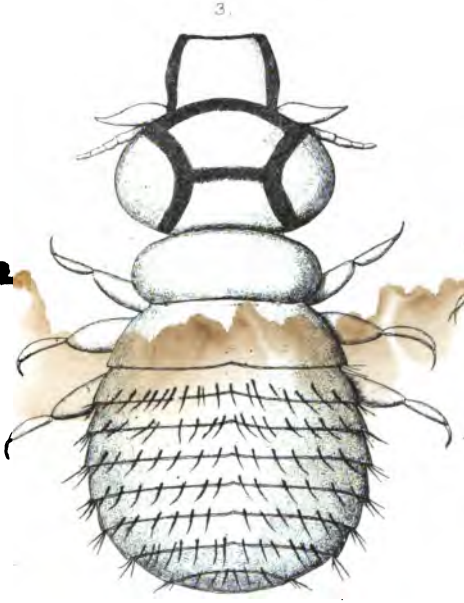
Docophorus Dennyii (mih) (Plate XCI., Fig. 3).—This species I have dedicated to the late Mr. Denny, of Leeds, to whom I am indebted for much kind advice and assistance in the study of the Anopleura. Colour tawny. Head triangular, clypeus produced entire; trabeculæ large, broadly truncated; antennæ rather long. Clypeus bordered by a chestnut line, with a transverse semilunar marking of the same colour, a similar one on the occiput; a broad irregular chestnut mark extends from the eyes to the prothorax. Prothorax transverse, angles rounded, metathorax transverse. Abdomen ovate, hairy; pale fulvous, with a chestnut border. Length, 3·173 mil. Habitat, *Prismites Mexicanus.*

Trichodectes leporis (mihi) (Plate XCI., Fig. 4).—Colour bright fulvous yellow, a dark chestnut spot at the eyes connected by a diagonal line with a line of the same colour on the occiput. Head suborbicular; clypeus rounded, vertex convex, lateral margin deeply sinuated; eyes prominent; antennæ small, last joint broadly clavate; prothorax transverse; metathorax not so wide as the head. Abdomen ovate, fulvous, hairy. Tibiæ clavate. Length, 2·538 mil. Habitat, *Lepus cannabinus.*

Clifton, *May*, 1871.







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New Parasites.

IV.—*On some Improvements in the Spectrum Method of Detecting Blood.* By H. C. Sorby, F.R.S., &c.

In the following paper I shall give a condensed account of what I have been able to learn in connection with this subject, and omit everything that does not bear directly on determining whether any stain is, or is not, due to *blood*. There does not appear to be any probability of our being able to decide by this means whether it is, or is not, *human*.

The spectrum-microscope used in these inquiries should have a compound prism, with enough, but not too great, dispersive power, or else the bands would be as it were diluted, and made less distinct. A combination of two rectangular prisms of crown glass, with a rectangular of very dense flint, and another of less dense, of such an angle as to give direct vision, turned towards the slit, as lately made for me by Mr. Browning, appears to be the proper medium, and has other important advantages. The cells used for the experiments should be made from barometer tubing, and be about one-eighth of an inch in internal diameter, and half an inch long, one end being fastened to a piece of plate glass with purified gutta-percha, like an ordinary cell for mounting objects in liquids. It is, however, a very great advantage to insert between the plate and the cell a diaphragm of platinum foil, having a circular hole about two-thirds of the internal diameter of the tube, fixed so that its centre corresponds with that of the cell. This prevents any light passing upwards that has not penetrated through the whole length of the solution, which is very important when using direct concentrated sunlight to penetrate through turbid or very opaque liquids. A small spatula made of stout platinum wire, flattened at the end, is very convenient for adding small quantities of the reagents; and they should be stirred up in the cells with a platinum wire, flattened and turned up square at the end, like a small hoe. The reagents commonly employed are a somewhat diluted solution of ammonia, citric acid, the double tartrate of potash and soda, used to prevent the precipitation of oxide of iron, and the double sulphate of the protoxide of iron and ammonia, employed to deoxidize; but in some special cases diluted hydrochloric acid, carefully-purified boric acid, and sulphite of soda are required.

The character of a stain varies much with its age, and with the nature of the substance on which it occurs. If quite recent, and if the substance has no immediate influence on blood, the stain would contain little or no colouring matter but hæmoglobin. This is easily dissolved by water, and when properly diluted—neither too strong, nor too weak—it gives the well-known spectrum, with two dark absorption-bands in the green. The addition of a very little

ammonia and a small quantity of the double tartrate produces no change, but on adding a small piece of the ferrous salt, about $\frac{1}{40}$th of an inch in diameter, and carefully stirring, so as to mix without much exposure to the air, these bands gradually fade, and are replaced by the single broad and fainter band of deoxidized hæmoglobin. When stirred up so as to expose well to the air, the two original bands of oxidized hæmoglobin can be seen again. On gradually adding a little citric acid, until the colour begins to change, these bands slowly fade away; and, if the amount of blood was considerable, a faint band would make its appearance in the red. When previously deoxidized, this solution may be turbid, but not so as to interfere with the result. The addition of excess of ammonia makes all clear again, but does not restore the original bands, or only to a slight degree, thus showing that a permanent change is produced by citric acid—the hæmoglobin is changed into hæmatin. This alone serves to distinguish blood from by far the greater number of coloured substances, which after being changed by acid, are restored by alkalis to the original state. On deoxidizing with the ferrous salt, we obtain the well-marked spectrum of deoxidized hæmatin, with one very dark and another much fainter band in the green, almost or quite invisible when the quantity is small. If too much citric acid or double tartrate had been added, this solution might be turbid; but, if all had been properly managed, it would be quite clear. Since the deoxidization takes place rather slowly, especially in cold weather, it is well to slightly stir up the ferrous salt at the bottom, completely fill up the cell, cover it with a piece of thin glass, remove the excess of liquid with blotting paper, and mix the solution by turning the tube upside down, over and over again. On reoxidizing the solution by stirring, the bands of deoxidized hæmatin disappear, and the two bands of hæmoglobin will probably be recognized, owing to citric acid not changing the original merely into hæmatin, but also giving rise to some methæmoglobin. The whole of these facts may be seen with a single cell, containing about $\frac{1}{100}$th of a grain of blood, and any experimenter should become quite familiar with them before applying this method to suspected stains in cases of importance. Very faint bands are best seen by lamplight.

On exposure to the air in a damp place, a blood-stain may be completely decomposed by the growth of mould, but when not thus destroyed it is partly altered into hæmatin. If, however, kept dry, the hæmoglobin gradually changes into a variable mixture of methæmoglobin, hæmatin, and a brown substance not yet much studied. This change takes place far more rapidly in the acid atmosphere of towns and houses, especially when gas is burned, than in the open country; but it does occur even in the purest air, and in glass tubes hermetically sealed. The presence of a weak acid in perspiration may also cause a stain on a worn garment to be completely changed

in a very short time, and the presence of a stronger acid on dirty clothes may at once alter the hæmoglobin into hæmatin.

On digesting in water a stain that has been kept until all the hæmoglobin has disappeared, the methæmoglobin dissolves. When the solution is sufficiently strong, this shows a band in the red, and two fainter in the green. The addition of ammonia removes that in the red, makes those in the green much darker, and develops a special very narrow band in the orange. When deoxidized this solution gives deoxidized hæmoglobin. Since methæmoglobin is formed at once from hæmoglobin by the action of a great number of different oxidizing reagents, and since it can be reconverted into oxidized hæmoglobin by slight deoxidization, I am inclined to look upon it as a peculiar oxidized modification. On adding a little of the double tartrate and of the ferrous salt to even a dilute solution from an old stain, the methæmoglobin is deoxidized, and the well-marked spectrum of fresh blood can be seen. If left too long, the spectrum of deoxidized hæmoglobin is developed, but on well stirring, that of the oxidized reappears, and the various other spectra may afterwards be obtained, as described above. That part of the stain, insoluble in water, which is chiefly hæmatin, may be dissolved in dilute citric acid or ammonia, and when deoxidized the spectrum seen to even greater advantage than when fresh blood is employed, because there is no general shading in the green, due to there having been methæmoglobin mixed with the hæmatin. We may thus obtain an excellent spectrum from a blood-stain nearly fifty years old. In very old stains all the methæmoglobin has disappeared, and sometimes even a considerable part of the hæmatin has been altered into another brown colouring matter, which does not give any well-marked spectrum.

When a blood-stain has been made sufficiently hot to coagulate the albumen, neither water, citric acid, nor cold ammonia will dissolve it, but by heating in dilute ammonia the hæmatin is easily dissolved, and may be detected either before or after concentrating the solution by evaporation. I may here say that the spectrum of deoxidized hæmatin can in no way be better seen than by deoxidizing a solution of fresh blood that has been boiled with dilute ammonia, which gives rise to a very pure hæmatin.

In applying these principles to the detection of suspected stains, it is desirable in the first place to examine a portion of the unstained fabric, to ascertain whether any colour is dissolved from it by water, and whether the solution has an acid, or alkaline, reaction. It is also important to ascertain whether colour is dissolved from the fabric by dilute citric acid or dilute ammonia, and if so, to determine whether this would in any way interfere with the recognition of blood by the processes described above. In the case of scarlet cloth and of some other red fabrics, much colour is dissolved out by

ammonia, but not by citric acid, which ought therefore to be used, whereas in other cases ammonia is the best solvent.

Unless the stain is faint, a portion should be soaked in a few drops of water in a watch-glass, the liquid squeezed out, allowed to stand a short time in the glass, so as to deposit any small portions of the fabric, and poured into one of the experiment cells. If the stain had been recently made, and had not been changed by any special action, a solution of hæmoglobin would be obtained, and the various spectra could be seen one after the other, as already described. If, however, the stain were a few days or a few weeks old, we should obtain a mixture of hæmoglobin and methæmoglobin, or the latter alone. The various spectra could then be developed, and compared side by side with those from fresh blood, to be sure that there is complete correspondence in the position and relative intensity of the bands. The residue insoluble in water should then be dissolved in dilute citric acid or ammonia, according to the nature of the fabric, and the spectrum of deoxidized hæmatin developed. If insoluble in cold citric acid or ammonia, hot ammonia should be tried, since the stain might have been so heated as to coagulate the albumen. If it be desirable to keep the specimen of deoxidized hæmatin for subsequent reference, the cell may be covered with a piece of thin glass, and, after removing the excess of liquid, the edge of the cover painted round with gold-size. When properly managed, such an object will show a perfectly good spectrum, even after many weeks.

If therefore we have a sufficient amount of a moderately old stain, we may easily see in succession the seven very different spectra of the following solutions:—1. Neutral methæmoglobin. 2. Alkaline methæmoglobin. 3. Deoxidized hæmoglobin. 4. Oxidized hæmoglobin. 5. Acid hæmatin. 6. Alkaline hæmatin. 7. Deoxidized hæmatin. If the amount was very small, only Nos. 4 and 7 would show distinct bands, and the rest would be characterized rather by their comparative absence; and it must always be borne in mind that Nos. 1 and 2 may be modified by the presence of unaltered hæmoglobin, No. 3 by that of dissolved hæmatin, and Nos. 5, 6, and 7 by that of undecomposed hæmoglobin or methæmoglobin.

It would be easy to obtain other preparations, and to see several other spectra derived from blood, but it appears to me unnecessary, since the above are so remarkable and unique in the manner in which they are produced, one after the other, especially by deoxidization and reoxidization on stirring, which seldom occurs in other colouring matters, that they afford as satisfactory a test for blood as could be desired, and still more so when we consider not only the general character of the spectra, but also the exact position of the absorption-bands, and that some are so most unusually distinct.

The above directions apply to simple cases, where the amount of material at command is amply sufficient, and the fabric on which the stain is found does not contain anything that makes the blood insoluble, or interferes with the various tests. I shall, however, now describe what should be done in cases which are made specially difficult by various causes. If the stain were very faint, from the presence of very little blood, or if the greater part had been removed by washing with water, it might be desirable not to divide the material, but to examine the whole at once. The stained portion should therefore be digested in a few drops of dilute citric acid or ammonia, and the presence of hæmatin determined, as already described. If faint and spread over a considerable surface, it might be well to digest in citric acid or ammonia diluted with much more water than would fill the experiment cell, and afterwards concentrate the solution by gentle evaporation. By this means blood could be detected, even when considerable effort had been made to remove it, and only a faint brown tinge left, just visible on white linen. There would generally be no difficulty in the case of a stain on cloth which had been sponged, for enough blood solution would be left in the fabric.

The presence of mordants in cloth or prints may require us to somewhat modify our proceedings, especially if the stain had been made wet, and to a great extent removed, so that we have only the dried-up solution of blood, thoroughly incorporated with the mordant. Certain kinds of brown cloth are of such a character, and about seven years ago portions of a wetted stain were sent by me to a number of the highest authorities in the detection of blood, and they said that neither they nor anyone else could recognize it. However, by proper care, I found that after a lapse of six years it could be detected by the spectrum method. The best plan was to digest a portion of the cloth in dilute ammonia, and to squeeze it well over and over again, with a pair of forceps, and finally with the finger and thumb, so as to obtain as much of the solution as possible. This was very turbid, but when deoxidized in the usual manner, and illuminated by concentrated light direct from the sun itself, the band of deoxidized hæmatin was quite distinct. When the cell was kept for a while, so that the insoluble part settled to the side, no band was visible, and therefore the hæmatin was evidently combined with the mordant. It will thus be seen that it may be most important not to filter or allow the insoluble matter to subside, but to overcome the opacity by means of a sufficiently intense light. If the sun could not be made use of, the lime or electric light would no doubt be the best substitute.

When fresh blood solution is agitated in a test tube with vegetable soil, and left until quite clear, the colouring matter is completely carried down with the earth. Dilute ammonia, however,

dissolves out hæmatin, and therefore, in testing portions of soil, they should be digested in considerably more of that solvent than will fill an experiment cell, and after the solution has become quite clear it should be concentrated by evaporation. The spectrum of deoxidized hæmatin may then be seen by following the ordinary method. The same process should be adopted in examining stains on clothes impregnated with earth or earthy dust, and marks on iron contaminated with much rust, if water will not dissolve out unaltered blood or methæmoglobin.

The importance of being able to detect blood-stains on leather was prominently brought before me by a case in which the trial of a suspected person depended on the nature of certain dark marks on his gaiters. The presence of tannic acid so completely mordants the blood, that neither water nor citric acid will dissolve it, and ammonia gives rise to a most inconveniently dark solution. If the stain is on the surface, and has never been wetted, a thin shaving should be cut off, so as to have as much blood and as little leather as possible, and the blood should be dissolved off without exposing the solution to the action of the leather itself. This may be accomplished by taking one of the experiment cells, nearly filled with water, bending the shaving, and inserting it into the upper part of the tube, so as to touch the water, being careful to arrange it so that the stain may be on the convex side of the leather, and in contact with the water. When a drop of blood falls on leather, many red globules are filtered out from the serum and left on the surface, and, when thus treated, they dissolve, and the coloured solution sinks at once to the bottom of the cell, without coming in contact with the leather. The various spectra may then be observed in the usual manner. This method would be of little or no use if the stain had been wetted, and for a long time I concluded that after such treatment it would be impossible to recognize blood. However, after many experiments, and after having again and again almost given up the inquiry in despair, I found that the difficulty could be overcome in a very simple manner. The best solvent for the insoluble compound of the colouring matter of the blood with tannic acid, is hydrochloric acid diluted with about fifty times its bulk of water. If stronger or weaker, the result is not so good. When a portion of unstained common brown leather is digested in this dilute acid, the solution is scarcely tinged yellow. On adding excess of ammonia, the colour becomes pale purple, or neutral tint, made deeper when the double tartrate and the ferrous salt are added, but remaining nearly clear. This gives a spectrum very dull all over, but without any trace of definite bands in any part. The depth of colour varies much with different specimens of leather. A portion of similar material soaked with wetted blood, gives a yellow solution, made brown-purple and turbid by the

double tartrate and ammonia, and remains so when deoxidized. The band of deoxidized hæmatin can however be distinctly seen with a light sufficiently strong to penetrate the turbid and dark solution. Before examining the suspected stain, it would be well to make out how much of the unstained leather could be used without giving too dark a solution, and to use no more of the stained. If the deoxidized solution be too turbid, the cell may be kept for a while horizontal, until the deposit has subsided sufficiently to allow the principal absorption-band to be seen; but it is not so distinct, when all has subsided, as though the greater part of the hæmatin still existed as a compound insoluble in dilute ammonia.

The presence of tannic acid in wood and other substances might make it necessary to employ a similar process, if the relative amount of blood were so small, that none could be dissolved out by water, or dilute citric acid.

Cases might occur when it would be necessary to decide whether blood were present, along with some other coloured substance, soluble in water. The method to be employed would depend much on the nature of this impurity. If it were a colouring matter, belonging to what I have described in former papers as group A, in which the absorption is removed by sulphite of soda, in an alkaline solution, there would be no difficulty in seeing all the spectra. Thus, for example, it is easy to add so much magenta to the solution of a little blood, that its absorption-bands are entirely hid; but a small quantity of sulphite of soda so completely removes the colour of the magenta, that the various spectra of the blood may be seen almost as well as if it had been pure.

The colouring matters of my group B that are most likely to occur, are those of fruits, and in them the presence of the free acid would be almost certain to have changed the hæmoglobin into hæmatin. The best plan would then be to add excess of ammonia, and, if the solution were made too dark, to dilute it with so much water that the strongest light at our command would show the green part of the spectrum sufficiently bright to prove that no absorption-band occurred there. On deoxidizing in the usual manner, the solution may be made somewhat darker by the presence of tannic acid, but the darker band of deoxidized hæmatin could be recognized without material difficulty.

By far the greater number of the colouring matters belonging to my group C are yellow and orange-coloured; and, since these chiefly absorb the blue rays, they do not interfere with our seeing the bands of the blood spectra, which occur in the green. Cochineal is one that requires special attention. The addition of ammonia to its solution in water gives rise to two bands in the green, which, though differing materially from those of blood, are yet so nearly in the same situation, that they completely disguise the presence of

a small amount of blood. However, on adding a small excess of boric acid, the bands of the cochineal are made more faint, and very considerably raised towards the blue end, so as to leave the red end of the green clear, whilst those of oxidized hæmoglobin are not changed, and that nearer the red end, if not both, can be seen perfectly well. By proceeding in the usual manner, there is no great difficulty in recognizing the darker band of deoxidized hæmatin.

Other special difficulties might occur in particular instances, but I trust that these examples will suffice to show how they may be overcome. I do not now know of any that require special remarks; and, as far as I am able to judge, we need never despair of detecting blood, so long as any hæmatin remains undecomposed. Fortunately it resists decomposition so well, that this would rarely happen in ordinary circumstances; but yet there are cases in which it does occur, as, for example, when acted upon by strong ozone, or other powerful oxidizing reagents.

It is quite possible that stained garments might have been washed, and some of the water employed might be obtained. If no soap had been used, this water could be examined in a long tube of thick glass, ten inches or more in length, and a quarter of an inch in internal diameter, permanently closed at one end with a circular piece of plate glass, and, when filled, covered over at the other with another glass. For examining solutions in such tubes a small pocket spectroscope, such as recently made for me by Mr. Browning, is extremely convenient, and suitable in every respect. If only two or three days old, the bands of oxidized hæmoglobin might be seen; but if the solution had been kept longer, and they could not be detected, it should be concentrated by evaporation at a gentle heat, and tested for hæmatin. If during evaporation any deposit be formed, insoluble in cold dilute ammonia, it should be dissolved by the aid of heat. When soap is used in washing off stains, the alkali soon changes the hæmoglobin into hæmatin, and the soap makes the solution inconveniently turbid and opaque. It is best in such a case to agitate the suspected soap and water with ether, remove it with a pipette, after the two liquids have completely separated, and repeat the process over and over again, with fresh ether, until the aqueous solution at the bottom has become quite clear and free from soap. It should then be concentrated by evaporation, and examined for hæmatin, as usual. Of course in all such cases it would be desirable to test the solution as soon as possible, lest decomposition should occur, but by these means a very small quantity of blood, that would show no colour, might be recognized within a week or two, but probably not after.

For the detection of blood in urine, a tube about ten inches long is very suitable. If turbid it should be filtered; but, since a

considerable number of red globules might be separated, the deposit on the filter should be dashed with a little water, and this solution either examined by itself, or added to the filtered urine. If the depth of colour in the ten-inch tube be so great, that the yellow end of the green part of the spectrum is absorbed, the urine must be somewhat diluted, or examined in a shorter tube. When the depth of colour is about an average, I find that by this means as little as $\frac{1}{10000}$th part of blood can easily be detected in fresh urine, which is equivalent to about one drop in a pint.

V.—*On the Cellular Structure of the Red Blood Corpuscle.*

By Joseph G. Richardson, M.D., Microscopist to the Pennsylvania Hospital, Philadelphia, Pennsylvania.

For many years after the magnificent cell theory was first accepted by physiologists, the doctrine of Schwann, who regarded the red blood disks as minute membranous sacs containing a coloured fluid, passed almost unquestioned; but of late, especially since more careful microscopic observations have become customary, it has been found that the supposed bursting of these little bladders, long looked upon as one of the strongest proofs of their cellular nature, does not take place, and at the present time some of our leading authorities, both in America and in Great Britain, assert positively that the coloured blood disks are non-vesicular, and deny any differentiation of their substance into cell wall and cell contents.

Thus, for example, Professor Austin Flint, jun., in the second volume of his great work on the 'Physiology of Man,' p. 116, remarks:—"The structure of the blood corpuscles is very simple. They are perfectly homogeneous, presenting in their normal condition no nuclei or granules, and are not provided with an investing membrane. A great deal has been said by anatomists concerning the latter point, and many are of the opinion that they are cellular in their structure, being composed of a membrane with viscid semi-fluid contents. Without going fully into a discussion of this point, it may be stated that few have assumed actually to demonstrate this membrane, but they have for the most part inferred its existence from the fact of the swelling, and, as they term it, bursting on the addition of water; and particularly, as it seems to me, to make the blood corpuscles obey the theoretical laws of cell development and nutrition laid down by Schwann. Their great elasticity, the persistence with which they preserve their bi-concave form, and their general appearance, would rather favour the idea that they are homogeneous bodies of a definite shape, than that they have

a cell wall with semi-fluid contents; especially as the existence of a membrane has been inferred rather than demonstrated."

Professor Lionel Beale observes, on p. 169 of his work entitled 'The Microscope in Practical Medicine:'—"The red blood corpuscle of man, and mammalia generally, consists of a mass of soft viscid matter, perhaps of the consistence of treacle, composed of hæmato-crystalline. It is, at least in certain states, soluble in water, but is only dissolved by serum and the fluid part of the blood very slowly. The outer part of this matter is of firmer consistence than the interior, especially in the older corpuscles. When the latter are placed in water the more soluble matter is dissolved, leaving the harder external portion." Dr. Beale further recounts sundry considerations which, he says, prove conclusively "that the red blood corpuscle is not a cell."

The distinguished French physiologist, Professor Ch. Robin, supports similar views, asserting, on p. 697 of 'Dictionnaire de Médecine, de Chirurgie, &c.,'*—"The red blood corpuscles are constituted of a homogeneous mass of globulin which is imbibed by or united molecule by molecule to the colouring matter, or hæmatosine, and a certain quantity of fat and saline materials. In mammals, the whole mass is homogeneous, and without any nucleus after the period when the human embryo, for example, attains a length of about an inch; but previous to that the globules, having a magnitude of from ·010 to ·011 of a millimètre, possess a little round granular nucleus. In all the oviparous vertebrates the globule, whatever its form, encloses a colourless spherical or oval nucleus, insoluble in water and acetic acid, while the red mass is soluble in these menstrua."

In the course of some researches of my own, however, "On the Detection of Red and White Corpuscles in Blood Stains,"† I have shown, first, that if a few drops of fresh blood be stirred up in many times its bulk of pure water, the coloured hæmato-crystallin will be dissolved, while a whitish insoluble residue, found under the microscope to be composed of transparent hyaline spheres about $\frac{1}{4300}$th of an inch in diameter, subsides to the bottom of the vessel; secondly, that if a fragment of dried blood-clot is exposed to the action of a current of fresh water, the hæmato-crystallin will, after a few minutes, be washed away, leaving an aggregation of what appear to be similar delicate cells, altered in shape by mutual pressure, but still preserving much of their rounded contour; and thirdly, by a calculation of the superficial area of the human red blood disk, based upon accurate measurements of its dimensions when magnified nearly 1800 times, that supposing a cell wall to exist, there would be almost precisely enough membrane contained

* E. Littré et Ch. Robin, Paris, 1865.

† 'Am. Jour. of Med. Sciences,' July, 1869.

in it to cover the surface of a sphere having the exact diameter of the red corpuscles when rendered globular by the action of water.

In one of my experiments on the action of water upon blood, as detailed in that paper, the development of Bacteria so obscured these supposed membranous cell walls that they became unrecognizable after standing seventy-two hours, so that, in order to determine whether their apparent insolubility could be overcome by prolonged maceration, I made the following additional investigations:—

On the 24th of March, 1870, I thoroughly stirred two fluid-drachms of blood into two fluid-ounces of fresh water, and allowed the mixture to stand undisturbed for forty-eight hours, when a light and flocculent deposit of a pale pink colour, occupying about half a fluid-ounce of the liquid, had fallen to the bottom of the vessel. On examination under the $\frac{1}{50}$-inch objective, this was found to be chiefly composed of very transparent spherical bodies, about $\frac{1}{4300}$th of an inch in diameter, which became beautifully distinct and quite visible with an ordinary $\frac{1}{4}$-inch when tinted by a minute portion of aniline solution introduced at the margin of the cover. In order to prevent the development of Bacteria, about two fluid-drachms of carbolic acid solution were added, and the mixture kept covered in a room of ordinary temperature for four weeks, at the end of which time the delicate colourless spheres were still distinctly visible, although they had a little further diminished in size, only measuring about $\frac{1}{5000}$th of an inch across.

From these various observations, it appears that human red blood corpuscles are composed of two different ingredients, the one hæmato-crystallin, of a crimson colour, and dissolving freely in water, the other of a whitish hue, and insoluble in water, even on prolonged maceration; but so minute are the blood disks in mammalia generally, that it is extremely difficult to determine the exact relation of these constituents to each other. It occurred to me, however, that investigations upon the large blood globules of reptiles might be more successful, and after numerous disappointments I procured, in November last, from a former patient near my late residence on Cayuga Lake, in Western New York, two specimens of the Menobranchus or Proteus, whose red blood disks, as far as known, with a single exception, exceed those of all other animals in magnitude, measuring about $\frac{1}{450}$th of an inch in length by $\frac{1}{650}$th of an inch in breadth, and actually visible, in a strong light, to the naked eye of a myopic person like myself. The gigantic corpuscles being about six times the diameter, and consequently 216 times the magnitude of those of man, evidently afford much better opportunities for the detection of their membranous parietes, if such exist; and in addition to this great advantage, I discovered, quite unexpectedly, in the course of my experiments upon them,

that their coloured portion possessed the remarkable property of crystallizing with great readiness *within* its envelope, and so enabling us to analyze, as it were, the corpuscle, by furnishing a singularly positive demonstration of the existence of a cell wall, totally distinct from the cell contents which undergo crystallization. These crystals, as often happens with those produced in the presence of organic matter, are frequently irregular, but their typical form appears to be that of a quadrangular prism, with dihedral summits, the angles sometimes being truncated. They may be easily prepared, as I have now done at least fifty times, by depositing a drop of blood from the Menobranchus upon a slide, allowing it to remain uncovered about ten minutes, or until a mere line of desiccation appears at the margin, and then covering it with a thin glass; on examination with a power of 200 diameters, numerous corpuscles along the edge of the drop where the liquor sanguinis has become most concentrated, will be frequently discovered to contain one, two, or more crystals; and under the most favourable circumstances of temperature and hygroscopic condition of the surrounding air, I have seen this process of crystallization go on until the contents of almost every corpuscle assumes the crystalline form, either wholly or in part, the cell wall being left in the former case perfectly colourless and transparent.

The effect of these crystals as they gradually elongate is very remarkable and interesting, being precisely that which would be produced by sticks of similar shape contained within an ordinary bladder partly filled with fluid; thus, for example, I have several times seen a single crystal, as if increased in length, thrust out the ends of the oval corpuscle, until the conjugate diameter of the cell became one-third greater, while its transverse dimension diminished to less than half its original magnitude, the nucleus being compressed closely against the side of the prism. Or in cases where one or more crystals happened to lie across the long axis, that decreased until the whole corpuscle assumed a lozenge-shaped or rectangular form, as in a very perfect specimen which I have mounted dry, the folded edge of whose capsular membrane may be seen supported by the crystals, like a washerwoman's clothes-line upon its prop.

It may in the first place be objected to this demonstration, that the appearance which it affords of a plicated membrane around the extremities of the crystals is caused by partial desiccation of the surface of the corpuscle while the specimen was being prepared; that such cannot, however, be the case, is proved by the fact that if, to blood freshly drawn from the reptile upon a slide, water is added, beneath the microscope we can produce an exosmosis of the coloured material into the diluted liquor sanguinis, leaving the same transparent cell wall, which becomes visible when

the cell contents are crystallized within it; and it is obvious that a membranous envelope, which is equally distinct under the opposite states of dryness and moisture, cannot be considered the result of either condition. Again, perhaps it will be asserted, secondly, that the appearances here presented might be simply the result of partial crystallization in such a drop of viscid material as Professors Flint and Beale consider the red blood disks, which drop, if the process were complete, would have entirely assumed the crystalline form; but I think I can quite destroy the force of that or any similar argument by the aid of other mounted preparations, some of them showing that well-developed crystals, which happen to lie in favourable positions, may include almost all the coloured portion of the corpuscle, without in the least affecting the contour of its cell wall.

I regret exceedingly that the difficulty of obtaining and preserving the Menobranchus alive, has prevented me from attempting to exhibit specimens of its fresh blood; but in the hope that other microscopists will repeat and correct or confirm my researches upon it, I am desirous of recording them and the conclusion which they seem to involve.

After a great many attempts, on which I spent altogether about eight hours' steady work, I have twice succeeded in cutting a corpuscle in two with sharpened needles upon a stage of the microscope, and beneath a half-inch objective, combined with a No. 2 eye-piece. On penetrating the vesicle with the edge of the needle, its coloured contents were instantly evacuated, and disappeared at once in the surrounding fluid, while the cell wall immediately shrunk together, and became twisted upon itself, and around the nucleus into a perfectly hyaline particle, which showed some tendency to adhere to the point of the instrument. It would therefore seem that the hæmato-crystallin was neither viscid nor semi-solid, and that the cell wall was structureless, and possessed only moderate tenacity, but of course the observations were too few in number to be accepted as conclusive.

When the corpuscles remained for two or three hours under observation, those which did not crystallize, often showed the wrinkled appearance figured by Hassal in his Illustrations, and described by Rollett, in Stricker's 'Handbuch der Lehre von den Geweben,' Zweite Liefrung S. 286, and which seemed to me due to the tendency of their colourless envelope, as the contained hæmato-crystallin condensed around the nucleus, to accommodate itself to the diminished contents of the cell by falling into folds frequently ramifying from the nuclear centre. When pressure was made by means of a mounted needle upon the covering glass, almost directly over a red disk, whose contents had undergone this contraction, the first effect was to round out the contour of the corpuscle, and

unfold the creases in its walls, the globule behaving as you might expect a bladder half full of water to do if you stepped firmly upon its centre; on continuing the process, however, no rupture of the walls could be detected, the contained fluid appearing to rapidly transude through its former envelope, which, on the needle being removed, collapsed to perhaps half its former size, and presented the aspect of a loose bag, almost without coloured contents, surrounding the nucleus. These changes were also examined under the $\frac{1}{25}$-inch objective, giving a power of almost 1200 diameters, by adjusting its component lenses, for a covering glass slightly thinner than that actually employed, and then cautiously screwing down the objective, so as to compress a blood disk beneath it; under this finely graduated pressure and high magnifying power, the apparent expanding of fold after fold, in the plicated wall of a perviously wrinkled corpuscle, became strikingly evident. After tinting the external portion of the red disk with aniline solution, and then applying considerable force to the covering glass, either by means of a mounted needle under a low power, or, with the extremity of a high objective itself, so as to empty out all the hæmato-crystallin, the shrivelled envelope could be traced after the removal of the pressure closely applied to the surface of the nucleus, and under such circumstances occasionally presented an obscurely granular appearance.

Sometimes a few of the corpuscles situated near the edge of the thin glass, and therefore most exposed to the action of the air, appeared, after three or four hours, to become cracked in various places from the circumference to their centre; those fissures seem to involve not only the cell contents, but also the supposed cell wall; although at first sight this phenomenon may be deemed inconsistent with the older theory, in regard to the structure of the red disk, yet I think that it can be explained by supposing that the hæmato-crystallin had in these cases undergone a sort of troubled crystallization, causing it to form a mass of tolerable firmness, which split into fragments as it became dry, and at the same time cracked its membranous envelope, just as a piece of muslin frozen fast to a lump of ice is sometimes broken with the fracture of the surface to which it is attached. In some instances, the delicate and transparent cell wall could be detected in the flaw of the hæmato-crystallin, its outer edge showing a concave line across the peripheral extremity of the fissure.

The addition of water to the fresh blood gave very interesting results, and occasionally afforded an admirable proof of the existence of a membranous envelope. The first effect of diluting the liquor sanguinis was to increase the thickness of the corpuscle, and under its further action the disk gradually became less elongated, until it assumed a spheroidal form, the coloured portion being

rapidly dissolved out, and leaving the nucleus and cell wall more distinctly visible. In one instance, a corpuscle which had become quite decolorized attached itself to some little mass of granular matter, so that it could be retained under observation while I set up currents beneath the cover by tapping the latter with a mounted needle. On changing the direction of these currents so as to strike the disk upon various parts of its surface in succession, I was enabled to satisfy myself conclusively that it possessed a bladder-like cell wall, perfectly flexible (now that it was no longer distended with hæmato-crystallin), and capable of being dimpled in, as it were, by the force of the current impinging upon any side until it applied itself accurately to the subjacent surface of the nucleus, thus furnishing strong evidence against the doctrine of a sponge-like stroma (or oikoid), as taught by Brücke and Stricker, being a constituent of the red blood corpuscle.

If water was allowed to flow in upon a specimen, whose disks had undergone the curious crystallization above described, diluted liquor sanguinis seemed to rapidly enter the corpuscles by endosmosis, and dissolved the contained crystals which generally assumed a foliaceous appearance, such as we often see crystals of triple phosphate put on, when macerated in alkaline urine. As the hæmato-crystallin dissolved, the natural colour of the corpuscle was restored; its walls, if they had been previously propped out upon the points of the crystals, reassumed their normal shape, and in some instances these shortened or broken crystals were observed to move freely in the cavity between the nucleus and the cell wall.

Before attempting to make any deduction from the above experiments upon the blood of the Menobranchus, it may not be amiss to refer briefly to the views entertained by most German writers in regard to the red blood corpuscle. According to Rollett, in Stricker's 'Handbuch' above referred to, p. 296, Hensen was led, from the apparent retraction of the cell contents from the membrane as seen in the blood corpuscles of reptiles, to ascribe to the red disks a protoplasm which, accumulated especially around the nucleus and over the inner surface of the envelope, was bound together by delicate radiating crossed threads, and in its interstices contained the coloured liquid cell contents (gefärbte zellflüssigkeit); but in the opinion of Rollett, this thing is untenable in view of the knowledge we have lately obtained in regard to the properties of protoplasm, from the researches of Max Schultze and Kühne.

Brücke, who has observed these appearances after the action of a 2 per cent. boric acid solution, pictures to himself in explanation thereof, in the first place, "a porous form element, consisting of a motionless, very soft, colourless, and perfectly transparent substance; further, he represents to himself the body of the corpuscle as composed of a living organism, whose central portion forms the

nucleus of all nucleated red blood globules, and is free from hæmato-globulin, while the remaining portion contains the entire mass of the latter. The last-mentioned part Brücke considers as lying in the interspaces of the porous mass, filling them completely, but at the same time forming a continuous whole with the non-pigmented portion. The colourless porous substance he calls *Oikoid*, all the remainder he names *Zooid;* and considers that, by the partial or complete withdrawal of the Zooid from the Oikoid, the occurrence of the above-mentioned appearances is explained. Stricker himself believes in the Oikoid of Brücke, but calls the remainder of the corpuscle its body (der Lieb)."

Recapitulating now the facts which I have detailed, militating against the views of Flint, Beale, and Ch. Robin, who hold that the red blood corpuscles of mammals are homogeneous drops of a jelly-like substance, we find first, that when human blood is diluted with pure water, the bi-concave disks in general gradually assume a bi-convex and finally a globular form, their coloured portion being entirely dissolved, sometimes in the course of a few minutes, while the transparent colourless constituent which retains the spherical shape is completely insoluble in water, even during the prolonged maceration of over thirty days; and second, that when a mass of desiccated corpuscles, such as occurs in a dried blood-clot, is washed with pure water, so as to remove all the hæmato-crystallin, the outlines of the compressed red blood disks may be readily detected on examination with a sufficiently high power; further, that in the red globules of the Menobranchus, which may be supposed to bear a more or less close analogy in their constitution to those of mammals, it is possible to analyze the corpuscle by separating the coloured cell contents from the colourless cell wall, either by puncture of the membrane, by crystallization of its enclosed fluid, or by pressure upon the corpuscle, forcing out its contents apparently through the pores of the membranous capsule in the same manner that quicksilver is strained by pressure through the sides of a buckskin bag.

In opposition to the theories of Hensen, Stricker, and Brücke, who consider the red blood corpuscles are made up of a colourless porous substance called Oikoid, and a coloured more fluid ingredient denominated Zooid, may be enumerated the following circumstances:—First, if, on the one hand, we consider that a porous substance of the definite bi-concave form, analogous to a disk of compressed sponge, exists, it seems impossible to account for this stroma assuming a globular shape when acted upon by water, since the full diameter of the sphere, when formed, is occupied by the least amount of distended matter, while the dimension, in which lay the greatest bulk of stroma previous to the addition of water, is actually diminished; on the other hand, any hypothesis that the porous

substance coalesces, on the removal of its hæmato-crystallin, into a jelly-like drop, is negatived by the fact that occasionally, as is also described by Professor J. C. Dalton, one side of a corpuscle, rendered colourless by water, fails to assume a convex form, being apparently sucked in by the other side, which becomes exaggeratedly convex, until the whole corpuscle resembles a bell, or more accurately a liberty cap, in shape, without any tendency to present the outline of a sphere. Second, the appearance of the specimens I have examined strongly indicates the existence of a dense membrane, thrown into folds around the extremities of projecting crystals, just as a loosened tent cloth would be around the point of a cane thrust against it from the inside; and further, the movement of the crystals, when partly dissolved, around the nucleus but confined within the corpuscle as described in the early part of this paper, both tend to show that the cavity of the corpuscle between the nucleus and the membranous envelope is quite unoccupied by solid matter. Third, the perfect freedom with which one side of the cell wall of a red blood globule from the Menobranchus when acted upon by water may float in until it touches the nucleus, and out again to its own place, will, I think, furnish conclusive evidence to anyone who sees it as I have done, against the existence of a porous substance which maintains the shape of the blood disk.

From these researches I therefore conclude that the older theory, which asserts that the red blood corpuscles of the vertebrata generally are vesicles, each composed of a delicate, colourless, inelastic, porous, and perfectly flexible cell wall, enclosing a coloured fluid, sometimes crystallizable, cell contents, which are freely soluble in water in all proportions, explains the physical phenomena presented by red blood globules far more satisfactorily than any other hypothesis which has hitherto been advanced; and, moreover, that the usual bi-concave discoid form of the corpuscles in most mammals, as well as the changes of shape which they undergo in fluids of greater or less specific gravity than the liquor sanguinis, becoming crenated in denser, and globular in rarer liquids, are such as to be perfectly explicable by the light of our present knowledge in regard to the laws of the exosmosis and endosmosis of fluids through membranes; the equilibrium of these forces being maintained in normal serum, and one or the other being rendered preponderant if the specific gravity of that fluid is disturbed.—*Transactions of the American Medical Association.*

VI.—*On the Use of the Nobert's Plate.*

By Assistant-Surgeon J. J. Woodward, U. S. Army.

I do not think the question of priority as to the resolution of the nineteenth band of the Nobert's plate, about which Mr. Charles Stodder makes such warm reclamations in the March number of this Journal,* possesses in itself enough general interest to make it worth while for me to add anything to what I have heretofore written on this head. If, however, as seems very probable, the Nobert's plate is to be much employed in ascertaining the comparative defining power of fine objectives, it is important that those who use it should have some reliable means of knowing whether they have actually resolved any given band; and such loose ideas appear to be entertained with regard to the matter in many quarters that it seems not undesirable to offer a few remarks on the difficulties involved, and the best means of overcoming them.

The satisfactory resolution of the Nobert's plate is, as is well known, complicated by the readiness with which spurious lines make their appearance parallel to the real ones, and simulating them more or less closely according to the character of the illumination employed. This difficulty is greater with the higher bands than with the lower ones, and with oblique light is more deceptive than with central illumination.

Three criteria for distinguishing the spurious lines from the true ones have been offered.

The first is the unaided judgment of the individual microscopist, who is supposed to be able instinctively to distinguish the false from the true lines without any special help.

The second is the enumeration of the lines in a measured portion of the band, and the comparison of the results attained with the statements made by Nobert as to their real distance.

The third is a count of all the lines in the band supposed to be resolved.

With regard to the first of these plans I must continue to think that it is utterly untrustworthy, and that should it unfortunately be generally accepted the plate would cease to possess any value as a measure of resolution, for individual enthusiasm would lead many to suppose they had succeeded, when in fact they were provided with utterly inadequate means.

It may be granted that an observer who has many times effected the true resolution of any given band will at length have its appearance so firmly impressed upon his mind that he will recognize it whenever he sees it as he would the face of a familiar friend, but this

* P. 118.

familiarity which all acquire with any appearance which they have many times reproduced, will only serve to mislead, if at the beginning spurious lines have been confounded with the true, for then the deceptive spurious appearance will be sought for as eagerly as though it were the true one.

A realization of this circumstance has led several eminent observers to propose a criterion of resolution which appears at first sight to meet the case, but which I must really think is more difficult and less accurate than the third method. This plan is very well described in the letter of President Barnard quoted in the article to which I have referred.* "When, for instance, I found that the value by micrometer of twenty spaces on the nineteenth band as counted, was exactly equal to the value by the same micrometer of ten spaces on the ninth band, I could not doubt that the nineteenth band was resolved." This method presupposes of course that the lines are ruled at exactly the distances Nobert intended, *viz.* those of the ninth band $\frac{1}{5000}$th of a Paris line from centre to centre, and those of the nineteenth $\frac{1}{10000}$th from centre to centre. It also presupposes absolute accuracy in comparing the portion of the two bands selected.

Now, the supposition that Nobert's estimate of the distances between the rulings is mathematically correct, appears to me highly improbable from many considerations; but when the attempt is made to demonstrate the precise degree of success attained, many difficulties are encountered. For example, as the ninth band contains twenty-seven lines and the nineteenth fifty-seven, if the distance of the lines of the nineteenth band from centre to centre is exactly half that given to those of the ninth, it follows of course that the nineteenth band ought to be broader than the ninth by the space of four of its lines. I have, however, been quite unable to satisfy myself that this is the case; repeated measurements of the nineteenth band of my plate inclining me to think rather that it is somewhat narrower than the ninth. It is indeed extremely difficult to make such comparison with the requisite precision, so difficult that I do not believe anyone could tell merely by the count and measurement of twenty lines whether he was examining the seventeenth, the eighteenth, or the nineteenth band. In the most careful and experienced hands, therefore, this plan offers at the best greater difficulties than a simple count across the band, and except in such hands it is only liable to mislead.

On the other hand, the third method is more positive, for the number of lines in each band being now known, a complete count gives results which cannot reasonably be questioned. But two objections have been made to this plan. First, that an objective of

* P. 123.

high angle may have exquisite definition combined with such curvature of field that a part only of any given band may be resolved at a time; and secondly, that in the case of the higher bands at least, a count of the whole band from edge to edge is so difficult as to be almost impracticable unless special costly and troublesome apparatus is employed.

The first of these objections falls to the ground if the actual width of the bands is considered in connection with the aperture of the objectives employed. It is asked, "If Nobert had covered a whole inch with the 112,000 and some odd lines, would anyone claim that all must be seen at once?" Now, the fact is that each of the bands on the plate is really only about the $\frac{1}{2000}$th of an inch in width; and the question is not whether an imaginary band of greater width could all be resolved at once, but simply whether the modern objectives as actually made have a field sufficiently flat to resolve from edge to edge a series of lines occupying a space the two thousandth part of an inch wide in breadth. I have already expressed my opinion on this matter, but desire here to offer a few considerations in its support.

My Powell and Lealand's immersion sixteenth, with the short eye-piece I generally employ on the plate, gives a field ·004 of an inch in diameter, or eight times the width of one of the bands. The Tolles' $\frac{1}{10}$th, belonging to the Museum, with the same tube and same eye-piece, gives a field ·008 of an inch in diameter, or sixteen times the width of a band. The Tolles' $\frac{1}{5}$th, belonging to the Museum, under the same circumstances, gives a field ·017 of an inch in diameter, or thirty-four times the width of a band.

With such an eye-piece only the central portion of the actual aperture of the objective is utilized; and I find that with the fifteenth band sharply in focus at one side of the field I get at the same time complete resolution of the fourteenth and thirteenth bands with both Powell and Lealand's immersion $\frac{1}{16}$th and Tolles' $\frac{1}{10}$th. I cannot, therefore, admit that the actual curvature of field is such as to prevent any given band from being resolved from edge to edge by an objective capable of resolving any part of it, and pass on to consider next the question of the difficulty of a count.

I have published elsewhere* what appeared to me to be a very easy and simple method of counting the lines. The circumstance that no one appears to have adopted it, on account probably of its requiring some special apparatus, induces me to mention some still simpler methods, which I have frequently employed with success. If after resolution is attained, a cobweb micrometer be substituted for the ordinary eye-piece, the well-known difficulties resulting from tremor will be encountered if any attempt be made to turn the screw and move the cobweb from line to line, as is ordinarily

* 'Quarterly Journal of Microscopical Science,' October, 1868.

done. But if instead, the blackened brass teeth which serve to record the movements of the cobweb be used simply as reference points to enable the eye to keep its place in counting across the band, a little practice will soon enable the observer to count the whole band with certainty and precision. The micrometer not being touched, there will be no tremor. It will of course be understood that a little strip of brass furnished with fine teeth, gummed to the diaphragm of an ordinary eye-piece, will answer the same purpose. A glass eye-piece micrometer will be found to impair definition too much. After a little experience, however, even the specks on the ordinary eye-piece and on the plate itself can be employed as reference points, and will render a successful count feasible, though not so easy as with the help of the economical contrivance I have described. Of course it is best in any case to begin with practice on the lower bands.

Having acquired the degree of skill necessary to enable him with such help to count a number of fine lines without losing his place, the conscientious student of the Nobert's plate has not, however, disposed of all the difficulties in his path. With such oblique light as is necessary for the resolution of all the higher bands with our present objectives, he will find a certain number of spurious lines on both sides of the band, and he will have to learn some method of determining where to begin and where to end his count. Nothing gave me greater difficulty in my earlier investigations of the plate, and I hope, therefore, that a short account of the practical results at which I arrived may prove of service to other microscopists.

If one of the higher bands of the plate be examined by an objective capable of resolving it, while it is illuminated by a pencil of insufficient obliquity, a mere tint or shade of the width of the band will be observed; with more oblique illumination a wavy irregular appearance comes into view; increasing the obliquity of the pencil still further, a series of lines make their appearance, occupying about the width of the band, but fewer in number than the real lines; finally, when the degree of obliquity necessary for actual resolution is attained, the true lines start into view, accompanied, however, by certain spurious lines on the margins of the bands. Four appearances of the bands are thus indicated, *viz.* a mere tint or shade, a wavy irregular appearance, spurious lines occupying the place of the band, and true lines with spurious lines on the margins.

The last two of these appearances require consideration.

A. *Spurious Lines occupying the Place of the Band.*—These are generally clean smooth lines, quite sharp, and much narrower than the apparent interspaces. They are well calculated to deceive the unwary, as they have often done. On a count, however, they will

be found too few; for instance, twenty to forty in the nineteenth band, instead of fifty-seven. The laws governing the production of these false lines and the relation of their number to the aperture of the objective and the obliquity of the illuminating pencil, have perplexed the most eminent students of optics, and are well worthy of future investigation. It is enough for my present purpose to state that while false lines of this character may be seen in the place of the true lines of the higher bands with objectives perfectly capable of resolving them with more oblique illuminating pencils, a similar appearance is often the best that can be produced with the most oblique pencils, if the resolving power of the objective is inadequate. These thin spurious lines differ in several particulars from the real ones of the higher bands. The latter are not smooth, they are irregular, they are thicker than the interspaces, they are wavy. The cutting tool has moved with a certain tremor; some of the lines are ploughed deeper than others, the distances from centre to centre are not always equal. Such inequalities might be expected in ruling such fine lines, and with adequate defining power they will readily be recognized.

B. *Spurious Lines seen on the Margins of the Resolved Bands.*—These require careful consideration by those who attempt to count the higher bands; but by selecting for study at first a moderately fine band, say the eleventh or twelfth, their characters can be learned, and the method of distinguishing them from the true ones mastered.

Suppose, for example, the twelfth band is under observation; when the pencil is sufficiently oblique to show the true lines, a series of fine spurious lines, closely resembling true ones, are seen adjoining the band, which, when the objective is in the lowest focal position compatible with definition, appear on the side from which the oblique light comes (as the microscope reverses, they are of course really on the opposite side). Of these spurious lines those next adjoining the real ones measure from centre to centre the same as the real ones, but the more distant ones grow gradually fainter and more separated till they disappear from view. On the opposite side of the band are a few coarser spurious lines, easily distinguished from the real ones. Such is the condition of things shown in my photograph of the nineteenth band. I do not think anyone could tell by mere inspection either of the photograph or of the image in the microscope, if the focal adjustment remains unaltered, which was the last true line, and which the first spurious one, on the side of the fine spurious lines; on the other side it is easy enough. But if now the direction of the light is reversed, the fine and coarse spurious lines change places, and what is still more important, a similar change can be effected by a mere change of focus. In fact, when the band is seen as above, if the fine spurious lines are on the

right and the coarse ones on the left, it will be found that on very slightly withdrawing the objective from the plate the fine and coarse false lines change place almost exactly as though the direction of the light was changed. This change is not a sudden one; fine spurious lines begin to be seen on the second side before they have all disappeared from the first, and there is no intermediate position such as may be attained with the lower bands, in which there are absolutely no spurious lines to be seen on the edges.

These results of a change of focus enable the observer to distinguish in the microscope the position of the last real line on either side, and thus to attain accuracy in his count; and if a successful photograph be compared with the object as seen in the microscope, there will be no great difficulty in determining, first on one side and then on the other, the portions of the picture which correspond to the true limits of the band. It was in this way that I determined the limits to be given to the enlargement of my photograph of the nineteenth band, which was distributed in every case pasted on the same card with a print from the unmodified original negative. The object of the limitation given to the enlargement was to make it serve as a representation of conclusions, readily obtained in the microscope by change of focus, but which the possessors of the photograph could not arrive at without repeating my observations, since the photograph represented of course but a single focal position.

If the first and last real lines are fixed in such a photograph by comparing it with the appearances in the microscope, the photograph will answer a useful purpose in verifying the count. Even if the observer is not sure as to this, if he can find, as he generally can, any reference points in the microscope and in the photograph by which fixed points in the band can be identified, a count of the intermediate lines will serve as a useful check. I have made use of all such aids in my study of the Nobert's plate, but I desire to say expressly that my statement of the number of lines in each band rests essentially on my counts in the microscope, and that I have repeatedly counted in the microscope the lines of each of the bands, from edge to edge. I may also make a single remark in this place with regard to our knowledge of the number of lines in the bands. I believe I was the first to state the actual number for the higher bands of the new nineteen-band plate. Nobert gave the distance of the lines apart in fractions of a Paris line, the actual width of the bands and the number of lines in each was not stated. From his figures the number of lines per millimètre and per inch has been computed. But the practical difficulty of measuring the width of a band with sufficient precision to deduce the number of lines from these figures has caused all the writers on the subject, whose papers I have seen, to observe a judicious reticence as to the actual number

of lines. If this has been anywhere stated, I should be glad to learn it, but my counts were all made in perfect ignorance of any enumeration but my own. Nobert, to whom I sent the photographs with my count, acknowledged the resolution in handsome terms. If there are any persons who still remain unsatisfied, I much hope that a better acquaintance with the subject will lead them to modify their opinion.

I pass by here the question of intermediate spurious lines between the real ones, since this concerns chiefly the lower bands of the plate.

It has, however, been suggested that if the criterion I have proposed be accepted, it ought to be applied also to the diatoms, and that it would invalidate all claims as to the resolution of these which are unaccompanied by a count and actual measurement. As to this, I would say that the optical conditions in the case of the diatoms are so different from what we have to deal with on the plate, that I cannot see that the one conclusion follows from the other.

The lines of the plate are minute grooves on the under-surface of the thin glass cover, and the point is to distinguish them from the spurious images to which they give rise. The striæ of the diatoms are the optical expression of sculpturings on frustules of silica. The appearance of lines is now generally conceded to be an illusion. What seem to be such are generally the optical expression of minute elevations, most probably hemispherical in shape, though the question of their form cannot be regarded as settled. These elevations are arranged in rows, to which the apparent striæ correspond. False lines of greater or less number are occasionally produced, and have in some instances been described as real ones, but this does not occur with facility on the frustules of most species, and on many does not occur at all. The question of the resolution of the diatoms, however, is too complex for further discussion in this place.

In closing this paper, I trust I may be pardoned a few remarks which appear to me to be warranted by the tone of Mr. Stodder's reclamations. He is quite right in his allegation that I have done "something more" than ignore his claims. I controvert them. I do so, first, because I have carefully tried a number of objectives made by Tolles, and have been unable to see with them any but spurious lines in the nineteenth band. Among those which I have tried is the much-talked-of $\frac{1}{10}$th and the new $\frac{1}{13}$th belonging to Dr. Josiah Curtis. Secondly, because Mr. Stodder has never yet offered any sufficient evidence that the lines he saw were not spurious also. He rests on a simple supposition, and in his recent paper supports this supposition by the mere opinion of several gentlemen to whom he has shown lines in the nineteenth band, but who, like himself, have taken no precautions to determine whether the lines seen were spurious or real.

I have never, however, expressed a doubt of Mr. Stodder's good faith in his claims, and will not do so now. Still I must call attention to some inadvertencies into which he has fallen. Thus on page 120 of his article he passionately denies having made any error as to the matter of counting fine lines, and quotes in proof the passage in his original paper, leaving out the part which contains the error. The whole passage reads, "either the micrometer or the stage must be moved, *and it is next to impossible to construct apparatus that can be moved at once the* $\frac{1}{100.000}$*th part of an inch and no more.*" His error, of course, consisted in supposing that if the micrometer is moved, its motions must correspond with the real distance of the lines, instead of the dimensions of the magnified image. The remarks on tremor which he introduces in this place have nothing to do with the question.

On the same page he insinuates that I have misrepresented the meaning of Professor Hagen's paper, but here his confessed ignorance of the language misleads him. I quote a single passage in reply. "Bis jetzt keines der objective von Tolles die 16 bis 19 Bande in Nobert's Platten vollig auflöst, was mit $\frac{1}{16}$th von Powell und Lealand gelungen."* I might quote several other examples of what I must hope is unintentional unfairness, especially his criticism of my photographs, of which I will only say that it contains conclusive internal evidence that he is unacquainted with the appearance of the true lines of the nineteenth band. But I am quite willing to leave this matter in the hands of conscientious students of the plate, and have neither time nor inclination to discuss his errors seriatim. I will close by a brief reply to his demand for my opinion as to Tolles' lenses.

I have always felt great admiration for the excellent workmanship of Mr. Tolles. I think his $\frac{1}{8}$ths and $\frac{1}{10}$ths will compare favourably with the like powers of the best makers, but I have not found that they excel them, and regard the claim that the $\frac{1}{8}$th of Tolles will do the work of the $\frac{1}{10}$th, or his $\frac{1}{10}$ths that of the $\frac{1}{16}$ths of other makers as utterly unfounded. I have long thought that if Mr. Tolles would apply himself to the construction of an immersion lens of shorter focal length than those he has hitherto made, the result would be gratifying to his warmest friends. Some time prior to the appearance of Mr. Stodder's paper, therefore, I sent through him an order for such an objective. When it reaches me I will endeavour to do it full justice; in the meantime I reply directly to Mr. Stodder's question, that I have two $\frac{1}{8}$ths by Powell and Lealand now in the Museum, each capable of resolving the sixteenth band of the plate, which is all I have ever been able to do with Tolles' $\frac{1}{10}$th or $\frac{1}{13}$th. With this I take leave of the subject.

* Max Schultze's 'Archiv,' Bd. vi., p. 217.

VII.—*On the Employment of Dammar in Microscopy.*

By Prof. Arthur Mead Edwards, New York.

In the London 'Quarterly Journal of Microscopical Science' for January, 1871, appeared an extremely interesting and valuable paper by Mr. Henry N. Moseley, "On the Use of Nitrate of Silver and Chloride of Gold in Microscopy," in which he calls attention to the use of "Dammar-firniss" by Stricker in place of Canada balsam as a medium with which to mount objects, the more especially histological preparations. And it is remarked that in this, Stricker's, laboratory, as well as in those of Brücke and Rokitansky, this medium has entirely supplanted Canada balsam. Mr. Moseley points out that well-made Dammar varnish possesses several advantages over the microscopist's old friend Canada balsam; and proceeds to point out that it is "clearer, more free from colour, and when used cold, as it always is, it dries quicker, though it is much thinner and more limpid." He also remarks upon the difficulty of obtaining good Dammar varnish in London, although the gum from which it is prepared is common enough.

As I have had some experience in the use of this material in microscopy, I will take the liberty of transcribing a paper read by me on this subject before the American Microscopical Society, April, 29, 1865, and which has never been in print as yet. Hereafter I will give some of my later acquired knowledge in this connection. The paper is entitled—

On a New Material for Mounting Microscopic Objects.

"Although I have called the material for mounting microscopic objects, which I am about to describe, new, it may not be so to some of the many students of the microscope; but, so far as I have been able to ascertain by inquiry among our own immediate members, it has not been as yet brought into use in this country; and as I am of opinion that it possesses in some respects superior characters, fitting it for the special purpose to which I have applied it, to Canada balsam, I venture to bring it to the notice of this Society, hoping that such of our members as will give it a trial will be as well pleased with it and obtain as satisfactory results as I have.

"I was more particularly drawn to ascertain if it might not be used in mounting microscopic specimens on account of a lengthened series of investigations undertaken for the purpose of ascertaining the improvements accomplished in the manufacture of modern objectives, and the consequent use of such glasses to determine, if possible, the character of the markings to be found upon the silicious cell walls of certain of the Diatomaceæ.

"Having then examined, with the various objectives which passed in review under my scrutiny, the specimens I possessed mounted in the two ways most commonly in use—that is to say, dry in air and in Canada balsam—it struck me that it would be well to try the effect of various media in assisting the performance of the instrument used; and, to that end, I mounted several specimens in different ways and in various varnishes and liquids, amongst which one may be particularized as very difficult to manage—that is to say, benzole. Amongst all the varnishes which I tried, I obtained the best results with a specimen of very old Dammar, which I was lucky enough to meet with in a small quantity, and which I was assured, by the person from whom I procured it, was of superior quality as a varnish, on account of its having been made some time, and at a period when spirits of turpentine and not petroleum naphtha was used in its manufacture; the latter material being used at the present time for dissolving gums and resins in varnish making, the war having rendered turpentine extremely scarce, as is well known.

"Old Dammar varnish, then, is the medium which I wish to recommend to the notice of the members of the microscopic fraternity; and as to the points in which it is superior to Canada balsam, I would state that its refractive power is such that markings which are with difficulty seen in balsam with a $\frac{1}{8}$th objective are with ease brought out sharply and distinctly when mounted in the Dammar with a $\frac{1}{10}$ths. It also dries almost immediately and without the use of much heat; in fact, much heat is rather detrimental, and I find the best method of procedure to be to dry the specimens of diatoms upon either the slide or thin cover as is desired, although the latter plan is the best, and slightly warming, drop upon them a very small quantity of pure spirits of turpentine, and, before it has all evaporated but has permeated throughout the mass of diatoms, to add the Dammar and bring the cover and slide, both slightly warmed, together. When mounting a number of specimens (say a dozen or so), as soon as we have put the cover upon the last the first is ready for cleaning, which can then be done with a small bradawl so as to remove the superfluous varnish, and the slide finished with turpentine. For cleaning slides the so-called "camphene" is the best material, as it is pure spirits of turpentine. Another great recommendation, as I consider it, to the use of Dammar for mounting microscopic objects is its great toughness, never becoming brittle by age, as is well known to be the case with Canada balsam. Besides, Dammar is commonly much lighter in colour than such Canada balsam as is generally to be found in the shops. When mounting specimens containing cavities —such as the *Isthmiæ*—perhaps even a little more care is necessary when using Dammar than Canada balsam; but when the effect

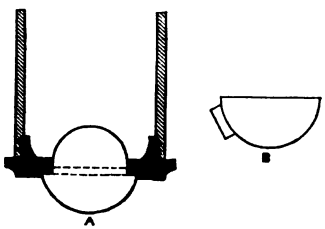
produced is taken into consideration, I am sure that microscopists will be willing to spare a little more time and labour over their manipulations so as to procure a superior quality of specimens."

Since this paper was written I have had much more experience in the use of Dammar varnish in microscopy, and mostly in preparing specimens of diatoms; but, all things considered, I think Canada balsam is the best material to use for that purpose. But as a cement, Dammar ranks very high, and I have put up a preparation of it for our principal dealer in microscopic objects and requisites, Mr. Miller, and he has found it extremely serviceable in the fastening together of glass as in constructing zoophyte troughs and growing slides. So my fellow-member, Dr. Arnold, favours very strongly its use as a cement in anatomical preparations. After having used it for some time and experimented considerably with this medium I consider that the reason why the first specimen I had was so clear was that it was thick. Dammar of good quality dissolved in coal-tar benzole and concentrated is very clear, otherwise it is milky until it thickens on the slide. Canada balsam, and, in fact, almost all solutions of resins in essential oils—*i. e.* varnishes—can be readily bleached by a few days' exposure to the sun in a closely stopped bottle. They are then much improved for use in microscopy.

VIII.—*Experiments on Angular Aperture.* By R. B. Tolles.

The following is given to illustrate the comparative available angle of dry and immersion objectives.

In the figure, "A" represents a plano-convex lens, nearly hemispherical, applied centrically to an objective at its front face. The objective used had an angle of over 170°.



When the hemispherical lens is thus applied to the objective an air space of course exists between the plane surfaces. On testing the angle only 80° (or at most less than 82°) was obtainable. Were the plano-convex removed, the angle *indicated* would be 170° upwards. This was verified at the time carefully.

When the air in this interspace is replaced by water, the angle becomes 100°, or a little more.

In this experiment the slide and cover are thrown out as of no importance to the solution of the question, *viz.* of the actual angular dimension of the pencil *traversing the object*, and transmissible by the objective.

It seems incontestable at all events that more than 82° of angular pencil *can* traverse the balsam-mounted object, and be transmitted by the immersion objective to the eye of the observer.

Incidentally to this proposition, the following is given when the object is actually *in situ* and well defined.

Thus, instead of the water in the above experiment, human blood was introduced between the plano-convex lens A, and the front surface of the objective.

As this necessitated, in order to bring the blood disks into view, separating the systems of the objective (by means of the cover adjustment) considerably, the apparent angle of this 170° objective, *i. e.* the angle taken in the ordinary way, proved to be only 128°. But the extreme ray transmitted when the blood was compressed by the plano-convex lens upon the front surface of the objective, proved to be less (a little) than 100°.

This form, just detailed, of the immersion objective is a "clinical" method, a year or two in use here, and wherein the front surface of the objective becomes the *stage* of the microscope, a glass "cover," or a lens as above, being applied to thin out the substance viewed, be it blood, urine, or other material fit to be thus put under view.

A natural sequence of all this is the application of such a plano-convex lens at the lower surface of the object slide, the primary object of it being to avoid the excessive reflexion that takes place at the immergent surface of an object-slide in all cases as now used.

Of course, in such an objective as used in these experiments, 170° upwards, the pencil incident upon the immergent surface of the slide must be to reach the full angle of the objective, very nearly parallel with the face of the slide. Immense reflexion is inevitable.

The application of the plano-convex lens to the under immergent surface of the object-slide allows the *extreme incident pencil* to enter at a perpendicular incidence very nearly. To be sure the convexity of the plano-convex lens has influence to modify this. But by placing upon the plano-convex lens a plano-*concave facet* lens (Fig. B), the incident rays meet a *plane* surface and pass on to the object without suffraction.

The increase of light in this latter case is necessarily large, and the influence of that increase upon the result, *i. e.* the appearance and demonstration of the object, is remarkable.

The convenience of using an incidence of only about 50° to 60° on each side of the axis, instead of nearly 90°, is evident enough. That this plano-convex lens fixed in the centre of the stage, perhaps preferably made achromatic, will be utilized as a condenser, there seems no doubt. In my own hands it seems to doubly demonstrate difficult tests.

Certainly the use of immersion condensers is abundantly indicated in the above simple experiments.

BOSTON, MASS., U.S.A., *May 24th*, 1871.

DR. HENRY LAWSON,
Editor.

BOSTON, *May 25th*, 1871.

DEAR SIR,—I yesterday mailed to your address a paper by Mr. Tolles on immersion objectives. I now wish to make one correction in that paper. The angle of the rays entering the objective with this arrangement is 110° instead of 100° as written. This makes the angle nearer to Dr. Pigott's statement, and farther from Mr. Wenham's. Please make the change when printing the paper, which I hope is in season for the July issue.

Respectfully,

CHAS. STODDER.

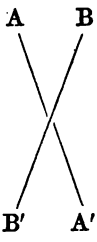
PROGRESS OF MICROSCOPICAL SCIENCE.

A Giant Gregarine.—We have just received from M. Van Beneden a copy of his memoir on the development of gregarines, in which the structure of *Gregarina gigantea* is fully described and figured. The pamphlet has reached us too late for any fuller notice at the present; but we shall dwell upon it more extensively in our next issue. It seems a most valuable addition to the literature of the subject, and it treats very fully upon the development of this very curious group. The author dwells upon Professor Beale's views on development of tissues. The work is published in the 'Bulletins de l'Académie royale de Belgique,' 2me série, tome XXXI., No. 5, 1871.

The Embryos of Calopteryx, Agrion, and Diplax.—One of the finest and most advanced memoirs that we have seen on these subjects is that just published in the 'Memoirs of the Peabody Academy,' by Mr. A. S. Packard, jun. It is extremely elaborate. After dealing at length with the subjects, the author thus sums up the characters:—Since the observations on Diplax were made, and abstracts read at the meeting at Burlington (August, 1867) of the American Association for the Advancement of Science, and published in the 'American Naturalist' for February, 1868, and in the 'Proceedings of the Boston Society of Natural History' (vol. xi.) for January 22nd, 1868, he has received, through the kindness of Dr. Alexander Brandt, of St. Petersburg, his admirable paper "On the Embryology of Agrion, Calopteryx, and certain Hemiptera." Brandt's studies were directed chiefly to the development of the embryonal membranes. His conclusions are: "1st. Calopteryx and Agrion are developed according to the type of the development as shown by Metschnikow to exist in the Hemiptera, namely, the germ or primitive band is internal to the yolk. 2nd. In those insects with an internal germ we need to distinguish an embryonal membrane, which is divided into a visceral and a parietal layer. 3rd. The visceral layer (veiled or plaited layer of Metschnikow) does not become united with the extremities, but enters, together with the parietal layer (amnion of Metschnikow), into the formation of the yolk sac. 4th. The formation of the yolk sac, together with the revolution or turning of the embryo on its transverse axis, consists in an independent contraction of the parietal layer of the embryonal membrane." As Mr. Packard's attention was directed to morphological points, he can only infer from the few data given above that Diplax and Perithemis have the same arrangement of the embryonal membranes, and that these membranes later in the life of the embryo form the yolk sac, through the contraction of the parietal layer of the embryonal membrane, as in Agrion, Calopteryx, and certain Hemiptera. As regards the changes of the embryo after the rudiments of the appendages have appeared, they seem in Diplax and Perithemis to be the same as in Calopteryx and Agrion. The embryo of Diplax is much thicker and shorter, corresponding to the shorter, more ovate egg. The attitude of the germ during its turning in the egg is identical with that of Agrion and

Calopteryx. Finally, Brandt's figure 19 may be compared with his figures 8, 8*a*, and 9, the yolk now being confined to a small area on the back of the embryo, which is now segmented and nearly ready to hatch, the claws being indicated, the eyes formed, the appendages partly jointed, and otherwise much as in the larva.

A New Method of producing Stereoscopic Effect.—In a recent number of 'Zehender's Monatsblat' there is an account of this new experiment of Listing, who has already done so much in physiological optics. It brings out stereoscopic effect with only one picture, which consists of figures arranged in a peculiar way, and seen with vertical double images. The simplest experiment is to view two lines crossing each other at an angle of about 30°, with a prism of 4° or 5°, its base vertical before one eye. No effort must be made to correct the vertical diplopia. If the prism be put before the left eye, its base upward, the line B B′ seems nearer to the eye than A A′. If the prism be turned with its base downward, and before the same eye, the line A A′ seems nearer, and B B′ more remote. It is found that with the base downward the prism must be weaker than when turned with the base upward. In gaining the effect by prisms so weak as these, no double vision is produced except for horizontal lines—the oblique lines appear to be only two. The same phenomenon may be produced in a common stereoscope by having two similar figures, and pushing one alternately up and down. Two rows of the same letters are arranged on a page like the limbs of the letter X, and viewed as above stated with a vertically deflecting prism; a sudden removal of one now takes place to a considerable depth, while this appearance is at once reversed on turning the prism around 180°. These curious effects can be best produced and understood by means of the diagrams accompanying the article.



The Red Blood-globule.—Dr. Richardson, of America, who has lately been inquiring into this subject, publishes some observations in the American 'Medical Times.' He desires to allude briefly to one of the minor points among his observations, which doubtless has been overlooked,—*viz.* that recorded to the effect that blood crystals of the Menobranchus, when partly dissolved, could be seen to move rapidly, and as if with perfect freedom, in various directions, between the nuclei and external borders of certain corpuscles. This fact appears to his mind much more consistent with the hypothesis of a cell wall enclosing fluid contents than with the doctrine of a homogeneous jelly-like constitution (Beale), or the theory of a crystalloid element "contained in an albuminous framework of paraglobulin" *firm enough to preserve the shape of the red disk* (Brücke, Stricker); and it seems to him the indication furnished by this circumstance resembles in kind the evidence which sudden dartings of a gold-fish across his vase would be that *he* was not imbedded in jelly or entangled within a net. Fully recognizing, however, the wisdom of caution against considering any one series of experiments (or, he may add, indeed, any one man's

unaided observations, however numerous) as "conclusive proof," and trusting, therefore, that these researches will lead others to investigate the subject and correct or confirm his results, he concludes his observations.

A Specimen of Diplograpsus pristis with Reproductive Capsules.—Mr. John Hopkinson, F.R.M.S., has recently described a curious graptolite. The chief peculiarity seems to be the presence of reproductive organs. These, which Mr. Hopkinson considers to be representations of the gonothecæ of the recent Sertularian zoophyte, are developed almost immediately opposite each other, from each side of the periderm and throughout its whole length. Though at equal intervals from each other, they are in no even numerical relation to the hydrothecæ, there being ten to the inch. They appear to have budded from the periderm at right angles to the hydrothecæ, and thus have caused the polypary to be unevenly compressed. The most perfect are pear-shaped in form, $\frac{1}{8}$th of an inch long; and at their narrow end, by which they are attached, about $\frac{1}{30}$th of an inch wide. They have apparently been bounded by a single marginal fibre, which is slightly thickened at its edges, and, where the pyrites are removed, has impressed a fine double groove on the surface of the shale. If the fibres were slender tubes, this appearance would naturally be presented; for their outer margins would offer the greatest resistance to compression. The so-called solid axis of the graptolite frequently presents a similar appearance. At the proximal end of the polypary these fibres only are preserved, the oldest or first-formed gonothecæ having fulfilled their function and perished. The distal extremity of even the most perfect is not clearly defined, the impression of the capsule in most cases becoming gradually less perceptible from the proximal to the distal end. Sometimes the capsules are irregularly ruptured, their torn jagged edges being distinctly seen, while one has split along its marginal limit, along the line of the marginal fibre, which appears to have parted abruptly near the distal end of the capsule at one side, and split acutely for some distance along the other side. This would appear to indicate that the capsule may be composed of two membranes joined together at their edges, through which the fibre, if it be not merely a tube formed by a kind of double marginal seam, has run. In no case can a distinct unruptured distal orifice be traced. The gonothecæ present other peculiar appearances. Towards their proximal end they are sometimes longitudinally corrugated or crumpled, or traversed by fibres which extend for some distance into the body of the polypary. Some are much twisted and bent about, occasionally overlapping each other. Between two which thus overlap, or perhaps only come into contact with each other, just at the point of contact and apparently within one of the capsules, are two minute young graptolites, one lying across the other. Each consists of a thin membrane, probably forming the first partially developed pair of hydrothecæ, a minute radicle, and a slender solid axis which is prolonged beyond the membrane. They are similar in form and proportions; but one is a little larger than the other. Its length, from the extreme point of the radicle to the distal end of the axis, is $\frac{1}{20}$th of an inch.

The membrane itself is about this length, and $\frac{1}{60}$th of an inch wide, tapering towards the proximal end. The smaller specimen is $\frac{1}{30}$th of an inch in entire length, and $\frac{1}{80}$th wide. If these young forms had not been in connection with a mature graptolite, they would have been considered to belong to the genus *Diplograpsus*, but it would have been impossible to refer them to any species. In their present position he thinks we may without hesitation infer that they are the young of the graptolite with which they are associated. That they have not yet entered upon independent existence we cannot conclude; for they are in different stages of growth, and young graptolites are frequently met with in a less advanced state than either; indeed, on the same piece of shale there are several young graptolites referable to the same species, and no more developed, some even less so.—*Annals and Magazine of Nat. History*, May.

When is a Blood Corpuscle in Focus?—Dr. Tyson has* a very interesting note, accompanied by a diagram, which we regret we cannot reproduce, on this optical and physiological subject. After explaining the diagram, he says it can easily be carried in the mind's eye, and at once the facts can be thought out without burdening with their recollection the memory, which is here peculiarly apt to be treacherous. Indeed, he said he could never himself promptly recall the circumstances under which the centre had been bright and the periphery dark, and *vice versâ*, until he had called to his aid this diagram. And that the exact truth is liable at least to escape attention, is seen in the circumstance that "in a volume no less highly valued than the seventh edition of Carpenter's 'Human Physiology,' 1869, is contained a misstatement of the facts. We find here, on page 200, the statement that the corpuscle is *rather beyond* the focus of the microscope when the *periphery* is *dark* and the *centre bright*, and *within the focus* in the opposite appearance—that is, when the *centre* is *dark* and the *periphery bright*. The reverse is correct. In the last edition of Carpenter (1868) 'On the Microscope,' however (pages 166, 167), we find the principle applied, and the fact correctly stated, though a few lines farther we find it asserted that the hexagonal areolæ in diatoms appear *dark* when the surface is slightly *beyond* the focus, though they are described as hexagonal *elevations*. If this latter be the case, then *they should appear dark when within the focus*, as is the case with the periphery of the corpuscle. So, too, on page 710 of this latter volume there is reproduced the same drawing referred to in the text-book on physiology, but with the description reversed, and therefore correct. The corpuscle is, however, described as in focus when the periphery is in focus, whereas we have presumed that the entire corpuscle is in focus when there is least shadow. Of the other text-books now within our reach, Dalton has it correctly on page 214 of his third edition; Flint, Kirke, Ranke in his 'Grundzüge der Physiologie,' and Rollett in Stricker's 'Handbuch der Lehre von den Geweben,' refer to the reversal of light and shadow, but do not state the circumstances under which it takes place; Marshall makes no allusion to it."

* Philadelphia 'Medical Times.'

Extraordinary Microscopy.—In a journal published in Philadelphia called the 'Medical Times,' which is remarkable for several able and interesting medical papers, we find an extraordinary communication (March No.) by Dr. Neulenz. The following quotation will give our readers some idea of this gentleman's opinions. We are a little surprised at their making their way to so great an extent as a column and a half in such a paper as the 'Medical Times':—"Having constructed a one-seventieth immersion objective, on a new principle, having 191° aperture,—the immersion liquid being fluoric acid,—and, for illumination, having invented a new eccentric parallelopiped, to be used with fluorescent rays exclusively, some remarkable results have been obtained. I take great pleasure in stating that, with regard to test-objects, all previous observers have been totally wrong in every particular, and that *Pleurosigma angulatum* is, in the first place, constructed on the plan of the Nicholson pavement, and, in the second place, that it is not a Pleurosigma at all. The most certain test-object is the *Newlenzia difficilissima*, a very rare and remarkable diatom, in which my one-seventieth with the parallelopiped shows four kinds of beads and six sets of cross-lines, one of which sets contains 147,229,073 lines to the inch: hence, by the well-known formula of Brewster, $\frac{d \cdot x}{d \cdot u} = \sqrt{o \cdot x \cdot p \cdot y}$, it is impossible that the undulations of light should pass without being previously deflagrated, and therefore no other lens can possibly show these lines, nor is it probable that this lens would with any other observer. The immense superiority of this test to Nobert's plate is apparent."

Note on Amphipleura pellucida.—Assist.-Surgeon Woodward, who may be said fairly to take first rank among American microscopists, has contributed a paper on this subject to 'Silliman's American Journal' for May. He says the attention of microscopists has frequently been directed, of late years, to the *Amphipleura pellucida* or *Navicula acus*, as a test-object well suited to try the defining powers of the very best object-glasses. The length of this diatom is stated by Pritchard as ranging from $\frac{1}{1400}$th to $\frac{1}{3000}$th of an inch. The average length is given by the 'Micrographic Dictionary' at ·0044 of an inch. The striæ, which are exceedingly difficult, were first described by Messrs. Sollitt and Harrison, who estimated them at from 120,000 to 130,000 to the inch. Their estimate has been adopted by the 'Micrographic Dictionary' and by the majority of modern writers who have referred to this test; but so many difficulties beset the resolution that few microscopists appear to have attempted to verify the original estimates. Indeed, most observers would seem to have been unsuccessful in their efforts to resolve the Amphipleura even with the best objectives, and some have gone so far as to deny the existence of any striæ upon the frustules of this species. Among the microscopists who claim to have seen the striæ, several would seem to differ from the original estimates of Sollitt and Harrison as to their fineness. Dr. Royston-Pigott, whose papers on "high-power definition" in the 'Monthly Microscopical Journal' have

recently attracted much attention, sets down their number at 150,000 to the inch. Dr. Carpenter, on the other hand, in the fourth edition of 'The Microscope and its Revelations,' expresses the opinion that even the estimates of Messrs. Sollitt and Harrison are too high: and we are told by Mr. Lobb ('Monthly Microscopical Journal,' vol. iii., p. 104) that Mr. Lealand has recently "succeeded in counting the Amphipleura lines and finds them 100 in $\frac{1}{1000}$th of an inch. A few months ago two slides of *Amphipleura pellucida* were received at the Army Medical Museum from Messrs. Powell and Lealand, and he succeeded in obtaining excellent resolution by the immersion $\frac{1}{16}$th of these makers. The frustules on the two slides were found to measure from $\frac{1}{170}$th to $\frac{1}{400}$th of an inch in length. Resolution could be satisfactorily effected and the striæ counted on any of them. He took eight successful negatives from medium size and small frustules, and verified the counts made in the microscope by counting the striæ on the glass negatives. He found the striæ on medium-sized frustules, say $\frac{1}{200}$th of an inch in length, counted usually from 90 to 93 striæ to the $\frac{1}{1000}$th of an inch; in that selected for the two photographs which were sent to the editors, the number was 91 to the $\frac{1}{1000}$th of an inch. Larger frustules exhibited rather coarser, smaller ones rather finer striæ. On the smallest frustules at his disposal, several of them only $\frac{1}{400}$th of an inch in length, he found no example in which the number of striæ exceeded 100 to the $\frac{1}{1000}$th of an inch. The striæ of these smallest and most difficult frustules do not then rival in fineness the nineteenth band of the Nobert's plate, as has been asserted by some; they compare rather with the sixteenth and seventeenth bands. After making the photographs, he extended his observations to a number of other slides of *Amphipleura pellucida*, including two of the original specimens from Hull, kindly sent to the Museum some time since by Mr. W. S. Sullivant, of Columbus, Ohio, and the example in the First Century of Eulenstein. He found that different slides varied considerably in the ease with which he could resolve them, chiefly as he thinks on account of the thickness of the glass covers, which in several instances did not permit the best work of the immersion $\frac{1}{16}$th. Perhaps, however, the markings on some frustules may be shallower than on others whose striæ count the same number to the $\frac{1}{1000}$th of an inch. In any event he has found, as yet, no slides the covers of which permit the $\frac{1}{16}$th to be approximately adjusted, on which it was impossible to resolve the frustules, and no frustules the striæ of which exceeded 100 to the $\frac{1}{1000}$th of an inch. The best resolution he was able to obtain by ordinary lamplight was not very satisfactory. He used therefore, during the investigation, direct sunlight, rendered monochromatic by passage through the solution of ammonio-sulphate of copper. A parallel pencil of such light was concentrated by the achromatic condenser, which was suitably decentred to attain obliquity. The same illumination was employed in making the photographs. He has since had the pleasure of exhibiting the resolution in quite as satisfactory a manner to several microscopists by monochromatic light obtained from the electric lamp.

NOTES AND MEMORANDA.

No Meeting of the Royal Microscopical Society this Month.—We beg to notice that, contrary to what has already appeared, there will be no meeting of the Royal Microscopical Society this month. It was intended to have held one, but the College being occupied on both the first and second Wednesdays in the month, the Council has been compelled to give way. Consequently, Fellows will observe that there will be no meeting of the Society held this month.

Contributions to the Journal.—We may state that various papers remain on hand. Among others, a long French communication, which cannot appear this month. We mention this fact to allay any anxiety which may be felt by persons who send contributions which do not immediately appear. We do our utmost in all cases to insert the articles which are sent to us, as well as the Reports of the local Societies; but of course cases occur wherein even for two or more numbers papers do not appear. We mention this fact merely to reassure our correspondents as to their communications.

CORRESPONDENCE.

Tolles' Stereoscopic Binocular Eye-piece.

To the Editor of the 'Monthly Microscopical Journal.'

Hobart College, Geneva, N.Y., U.S.

Sir,—I wish to correct as widely as possible a statement in regard to the stereoscopic binocular eye-piece, which attributes the invention to me instead of Mr. Tolles, to whom really the whole credit belongs. Dr. Carpenter* has made, unintentionally, such a statement, and it has been copied by others.† I doubt not it will be rectified in future editions. This misstatement was unknown to me until within a few weeks. If it had been known I should have made the correction promptly. It is not difficult, perhaps, to account for the mistake, inasmuch as I first exhibited this eye-piece in England at the soirées of the Microscopical Society and the Royal Society, and to numerous individuals, among them Dr. Carpenter himself, who expressed his satisfaction at its performance. Mr. Ladd, the well-known philosophical instrument maker, 11 and 12, Beak Street, Regent Street, had it for some time in his possession, and indeed made one, which however was much inferior to Mr. Tolles', as Mr. Ladd had not time to determine the proper curves, if indeed the lenses were achromatics

* 'Microscope,' 4th ed., p. 35. † 'The Microscope,' by J. Hogg, 7th ed., p. 119.

at all. He understood, however, that it was Mr. Tolles' eye-piece; and I have by me the original "exhibitor's card" at one of the soirées named, reading distinctly, "Tolles' binocular eye-piece, exhibited by Prof. Smith." I feel very anxious to have the mistakes corrected.

The first eye-piece of this form which Mr. Tolles ever made was purchased by me, and I gave some account of its performance and peculiarities in the 'American Journal of Science,' July 1864, p. 111. And, in the same journal shortly after, Mr. Tolles himself described its construction. Now, although we cannot expect everyone to read an American journal, or to be posted in all that is done this side of the Atlantic, even in the microscopical line (except to notice the trash, *e.g.* the mean little sheet issued by manufacturers and sellers of the Craig !!! microscope, recently attempted to be palmed off as the organ of the Illinois Microscopical Society), it is a little surprising, considering the length of time these two articles have been published in a most prominent journal, that such a blunder should occur. Of course it is inadvertence. Dr. Carpenter, I am quite sure, is ready and willing to do full justice, and in so doing it will be proper to state the *real principle* of the eye-piece in question. It is not, as he has stated, *l. c.*, merely an arrangement of prisms similar to MM. Nachet's, for in reality the prime part of the eye-piece may be this, or Riddell's, or Wenham's; and, in fact, in the first eye-piece made for me was different from either of these. Mr. Tolles finally—partly at my suggestion, though I believe he had already decided upon it—adopted the Nachet form, and he claims nothing for this. What he does claim, and is justly entitled to claim, is the construction of a first-class achromatic *erecting eye-piece*, and a division of the pencil, for stereoscopic vision, at, or very near, the point of crossing of the rays in such a combination. Now, it is well known that the difficulty in using the Wenham, or Nachet arrangement with high powers, arises from the necessity of dividing the pencil so far behind the objective, a difficulty which it seems cannot be got over, except upon Mr. Tolles' plan, *viz.* making a secondary image, and dividing the pencil here, or near the point of crossing of the rays. The binocular eye-pieces invented by President Barnard, and by myself, are simply binocular, like Powell and Lealand's arrangement for high powers, though superior as to equality in illumination of the two fields, they are not stereoscopic. Perhaps the fact of my having made such an eye-piece, and published an account of it, as also Dr. Barnard's notice of it, in his report upon the Paris Exposition, may have assisted to mislead in attributing the really stereoscopic binocular of Mr. Tolles to me. If I had been the originator of *this* eye-piece, which is yet destined to replace most binoculars, I should feel I had contributed a much greater boon to microscopy than anything I have yet done. The instrument, as made now by Mr. Tolles, is very perfect; the loss of light is trifling, easily remedied by a little more illumination. The loss in definition is not so much as in the Wenham and Nachet forms; not merely from the care with which Mr. Tolles works the prisms, but owing to the much shorter distance which the reflected ray has to travel. This part of Dr. Carpenter's objection is practically without

force. The eye-piece is expensive. Not only is it, at least so far as regards all below the prisms, perfectly achromatic, but of very peculiar and perfect construction, and as to liability of derangement, I can safely say it is as firm as any binocular arrangement now known, and easily adjusted if it should become deranged. I am told that M. Hartnack has made a similar eye-piece. During the Exposition I placed Mr. Tolles' eye-piece in his hands for inspection, and it remained with him for several days. I cannot say whether he copied it, or whether his arrangement is different in principle, or the same, as I have not seen it. M. Hartnack appeared to me to be far too honourable a man, as MM. Nachet, to whom also I showed it, most certainly were, to take any credit for an invention truly belonging to another.

H. L. Smith,

Formerly of Kenyon College.

The French Erecting Prism a Camera Lucida.

To the Editor of the 'Monthly Microscopical Journal.'

Reading, *June 5th*, 1871.

Sir,—Needing greater amplification for minute dissections and diatom selection than I could obtain from the simple lens of the dissecting microscope, and unable to overcome life-long habit and to reverse every movement which the inverted position of the object under the compound instrument necessitates, I applied to Mr. Curties for an erector, who furnished me with a French eye-piece erecting prism, which effects my object very completely.

I find, however, that it subserves another purpose, for which certainly it was never originally designed, and, so far as I know, has hitherto remained unsuspected—that of an effective camera lucida, with which very satisfactory outlines can be made, either in the vertical position of the microscope, on a screen placed behind it, or in the ordinary horizontal one (by far the most convenient in practice), on a paper extended below it on the table, the usual focal distance of ten inches being in both cases maintained. *Probatum est!* But how is the image produced and the sketch obtained? Backed by metal plates, with the image of the object on the stage refracted by the prism through the small circular opening of the front plate, on to the retina of the observing eye *alone*, it is obvious that the image seen on the screen or on the table *cannot be due to any reflexion*. It must, therefore, as suggested by my friend and neighbour, Dr. Shettle, be illusion only—a spectre, a brain phantom—though definite and capable of delineation. Have we not here a clear demonstration of the physiological fact, "that though the image of an object be impressed on the retina of one eye *exclusively*, through the decussating nerve fibres of the optic commisure, it makes an equal visual impression on that of the other"? It may be objected, I know, that the one eye sees the image, the other the point of the pencil only; that the one records what the other

sees, and that it is merely another application of the well-known method of taking rough micrometric measurements from a scale placed on the stage by the side of the object. But does not this also imply the transference or interchange of the retinal impression made on the one eye to that of the other, with an equal visual result? Or, would not the inference necessarily follow that it must be the *purely cerebral perception* which we subject to mechanical measurement and delineation? Can such a solution be accepted?

The proof, however, that I here offer, the more plausible explanation of the observed fact is, I believe, readily afforded by a simple experiment. Hold before the non-observing eye a lucifer-match, or something similar, in the same plane as the prism, an inch or so on one side of it, the spectral image of the match will then be seen *interposed* between the observing eye and the image of the object on the stage refracted into it by the prism.

To you, sir, as a most competent authority, I submit the matter.

Yours, &c.,

J. G. Tatem.

Linear Projection and Rotifers.

To the Editor of the 'Monthly Microscopical Journal.'

Dear Sir,—Permit me to express all due thanks to Mr. Charles Cubitt for his proffered lessons in linear projection, as applied to the delineation of rotifers. And respectfully to decline. That he should select me as his "frightful example," and take so much trouble with the "sole object" of assisting me, is a flattering mark of attention to excite some gratitude, as it certainly does considerable surprise. While giving full credit for excellent intentions, I am constrained to notice some misapprehensions and mistakes into which he has unwittingly fallen. He assumes that some, or all, of the figures illustrating my description of *Œcistes intermedius* are incorrectly drawn, because he fails to reconcile one with another, in accordance with *his* reading of the laws of linear projection. Well, so much the worse for Mr. Cubitt, for I can assure you that the portraits in question are most excellent—thanks to artistic skill not my own—and, without exhibiting affectation of extreme correctness, are yet perfectly amenable to the rules of linear projection when properly applied. It is almost impossible, without a diagram, to show the source of Mr. Cubitt's error and its correction, but a hint or two may suffice. The disk in the ventro-lateral view is inclined at a sharp angle, while those of the other two are foreshortened, and not so much turned dorsally. Thus, any one set of projection lines applicable to the former should, and do, break down with the latter. The side view was not "created" from the back and front views, but drawn under the microscope from another and a finer specimen, without reference to compasses or ⊤ squares, and yet with a result almost photographic in its truth.

When we have to represent an active, elastic, ever-changing creature from different points of view, I cannot think it well to treat it as a petrifaction, and draw it in precisely the same attitude in each view. It generally happens that a variation in the position brings into sight some organ or characteristic feature of the animal, and these surely should not be sacrificed in order that rigid rules may find easy application by beginners.

As Mr. Cubitt has put on a high power, his finest illumination and aplanatic searcher to view the supposed mote in *my* eye, allow me gratefully to note—without any magnifying—the rather obvious beams in his own. His Fig. 9, Plate LXXXIII., is called by him the "dorsal view" of Melicerta; it is really the ventral aspect.* Nor is this blunder a mere slip of the pen, for it is repeated, and with an imaginary "dorsal lobe" applied to every rotifer he speaks of, causing a confusion impossible to be worse confounded. He may—probably does—mean the ciliated prominence in the ventral aspect as his "dorsal lobe"; but fancy a describer of the elephant giving the trunk as an appendage of the back! Again, in an attempted partial classification, Mr. Cubitt calls Lacinularia a "free" form; it is no more or less free than is Melicerta (called "fixed"), it has its little fling in very early life, then takes a weed, and settles down quietly while yet a youth, like all its builder kindred (except Conochilus). Nor, by any stretch of imagination, can Cephalosiphon be recognized as a Philodine. Had Mr. Cubitt ever seen the former it would have been impossible for him to group them together.

Fain would I follow this gentleman in his more ambitious writings concerning rotiferous nerves, brains, osmosis, and sundry laws of physiology, but he occupies most boldly the ground on which, with angelic timidity, I "fear to tread."

I remain, dear Sir, yours faithfully,

H. Davis.

Coal Plants.

To the Editor of the 'Monthly Microscopical Journal.'

Goats Shaw, Oldham.

Dear Sir,—It will be remembered by some of the readers of this Journal that Professor Williamson read a memoir some time ago, before the Manchester Philosophical Society, on a new fossil fruit found by me in one of the lower coal-seams of the Lancashire Coal-field: this fruit he described as belonging to his new plant called *Calamopetus*.

Since then he has read another memoir before the above Society on another but very different fossil fruit found by me in the same coal-seam. I believe this memoir is now in the press.

I have been very fortunate in finding another coal fruit, very dif-

* See Gosse in 'Phil. Trans.,' Pritchard, and elsewhere.

ferent from either of the above. The sporangia in the two first fruits are protected by bracts, which pass between the sporangia from the axes. When they reach the outside, they ascend and overlap each other, as is seen in *Lepidostrobus*. This fruit I have just found seems to be void of these bracts altogether, and appears to be a *naked* cone. The spores are those of a Calamite, and the sporangia are very numerous and densely packed together. If the above features are borne out by the cutting up of this specimen, it will be made public in a future memoir.

I am yours respectfully,

JOHN BUTTERWORTH.

PROCEEDINGS OF SOCIETIES.*

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *June* 7, 1871.

W. K. Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

It was announced in reference to the proposed meeting in July that as the College authorities were unable to grant the use of the rooms in consequence of their being occupied for examinations, &c., the meeting could not conveniently be held as proposed.

A list of donations to the Society was read, and a vote of thanks passed to the respective donors.

A letter was read from M. Ernst, of Caraccas, describing a specimen of diatomaceous earth found in that locality.

The Secretary stated that Mr. Slack had added some notes to this letter. He found the earth rich in specimens; but up to the present nothing decidedly new had been detected.

The Secretary announced that it had been proposed in council to confer the honorary Fellowship upon a distinguished microscopist, Dr. Maddox, who had contributed largely to the 'Transactions' of the Society. As Dr. Maddox resided in the country, the Council thought that they would not be doing wrong to confer this mark of respect upon him.

Mr. J. Hogg read a paper "On Mycetoma, or the Fungus-foot Disease of India."

Mr. Stewart said he had had an opportunity of examining a speci-

* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'

men of this foot. The appearance which it presented was precisely that of caries of the tarsal bones. On the surface of the foot were a number of openings and sinuses leading down to the broken-down remains of the bones. A longitudinal section of the foot revealed the condition of the bones, which were simply indicated by some soft masses, which readily broke down with the slightest touch. The bones were much blackened; and on cutting through them for examination under the microscope, he found a large quantity of mycelia. He did not observe any fungi in the skin. The bones were literally one mass of fungus, occupying all the cavities.*

In reply to a question from the President, Mr. Stewart said there could be no doubt about the thing found being a true fungus. No tissue in the body ever presented any resemblance to the dark strips so characteristic of fungus. It was a true vegetable that he had seen; there was nothing in the human body like it.

The President thought the fungus was not the cause of the disease; but that the disease previously existed, the form which it ultimately assumed being a rapid downhill degeneration. The changes which the skeleton underwent in the course of nature would lead to softening of the bones, and degeneration would naturally follow where there was any tendency to disease.

Mr. Stewart said he should mention one fact in which the disease described differed from caries of the bone, *viz.* that in the cavities amongst the tissues which contained the fungi, instead of there being larger granulations the cavities were perfectly free from them.

The President remarked that it would seem as if there was no power of reparation in the bone, or capability of producing any new tissue, but it was simply the existing tissue going from bad to worse.

Mr. C. Brooke said he could not conceive that any spores of a fungus should make their way through the surface of the foot into the interior of the bone, so as to be developed at the depth named, and so as to cause the disease. They might run along the sinuses made for them; undoubtedly the existence of sinuses involved the supposition that there was pre-existent disease.

The President remarked that no number of spores would affect any healthy foot.

A vote of thanks was then given to Mr. Hogg.

Dr. Braithwaite read a paper "On the Structure of Bog Mosses."

A vote of thanks was then passed to Dr. Braithwaite, who took occasion in his acknowledgment to remark that the new school of bryologists now studied mosses entirely by the leaves, which are so definite and unchangeable in their characters, that every moss can be distinguised by its own peculiar leaf.

The meeting was then adjourned till the 4th October.

Notice was given that the library and reading-room would be closed during the month of August.

* These masses have since been examined by several independent observers, none of whom have been enabled to discover fungus in their composition.—J. H.

Donations to the Library and Cabinet from May 4th to June 7th, 1871:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*W. W. R.*
Journal of the Royal Institution, No. 6	*Institution.*
On a Specimen of *Diplograpsus pristis* with Reproductive Capsules. By J. Hopkinson, F.G.S.	*Author.*
The Medical Directory for 1870	*J. W. Stephenson, Esq.*
The Peerage and Baronetage for 1870	*Ditto.*
Journal of the Linnean Society, No. 56	*Society.*
The Canadian Journal, No. 73.	
Five Slides from Tasmania	*E. D. Harrop, Esq.*

John Stuart, Esq., and R. G. McLeod, Esq., were elected Fellows of the Society.

WALTER W. REEVES,
Assist.-Secretary.

CROYDON MICROSCOPICAL CLUB.

At the meeting on Wednesday, February 15th, the President, Henry Lee, Esq., in the chair, Dr. Strong read a paper "On Bone Structure," which the President afterwards described as clear, concise, instructive, and one on which he could desire the contributions of other members to be modelled. Dr. Strong took as the heads of his subject:—1st, the appearance seen in ordinary transverse and longitudinal sections of bone; 2ndly, the varieties of bone, and in what they differ; 3rdly, the development of bone, how it is formed, how nourished, and finally, its chemical composition. A section of the human thigh or arm bone (taken from the centre of the shaft, because the ends present a different appearance) shows that it is covered by a membrane, the *periosteum*, and that there is a hard layer or ring of bone surrounding a central cavity containing the medulla, fat or marrow—this hard layer or compact tissue, as it is called, being lined on the interior by a membrane, termed the medullary membrane, which serves to support the fat and the nutrient vessels. A thin slice of the "compact tissue" placed under the microscope, exhibits, 1st, some ovoid or circular holes, called "Haversian canals," after their discoverer, Clopton Havers, surrounded by concentric rings. Among these rings, or *lamellæ*, are dark specks, called *lacunæ*, which are in reality minute spaces. From these radiate still more minute pores or tubes, called *canaliculi*, which communicate with those from neighbouring *lacunæ*. The Haversian canals are the means by which the blood-vessels are carried into the interior of the bone. Of *lacunæ* and *canaliculi* there is little to be said beyond the fact that they are arranged concentrically round their own Haversian canals, and that the *lacunæ* occupying the outer margin of the ring have their *canaliculi* only on that side nearest the Haversian canal. Dr. Strong alluded to the views of Dr. Lionel Beale, on the nutrition of bone by particles of

protoplasm, occupying the *lacunæ*, and conveyed in minute streams through its structure by the *canaliculi*, and then described the cancellate structure of those portions of a bone where considerable strength is required, combined with lightness. Describing the development of bone, he said that it is formed in two ways—from membrane and from cartilage. The bones of the skull are an example of the first method, being first deposited in the centre, and then radiating in all directions. Where bone is developed from cartilage, it commences from several centres, which, by the constant addition of earthy particles, gradually approach each other till they meet. After some additional observations of the nutrition of bone by the *periosteum* on the exterior and by the medullary membrane on the interior, the lecturer proceeded to consider the chemical constituents of bone; and in the course of his remarks mentioned that in adult life human bones are composed of one-fourth of animal and three-fourths of earthy matter; that in infancy these constituents are about equal, whence their liability in children to bend rather than break; whilst in old age, the earthy matter being greatly in excess (seven to one), bones are much more liable to fracture. Dr. Strong concluded by recommending his hearers to adopt Mr. Squeers's method of investigation, by eating the flesh of their next leg of mutton, and converting the bone into a microscopic object.

The thanks of the meeting having been given to Dr. Strong for his interesting communication, the President remarked that microscopical comparison of the size, form, and proportionate number of bone cells, and the *canaliculi* radiating from them, had been productive of very important results in contributing to our knowledge of some of the remarkable animals which existed on the earth in former ages. As an example of this, he would mention that about the year 1845 Professor Owen read a paper before the Geological Society, on some *Ornitholites* (fossil remains of birds) from the chalk, the bone especially described being a portion of the humerus of a (supposed) longi-pennate bird. Subsequent discoveries of similar bones led Dr. Bowerbank to believe that these so-called bird bones were those of the great flying reptile, the Pterodactyl. By one of those strange coincidences of thought which not unfrequently happen in scientific investigation, Professor Quekett was also impressed with their similarity, and he and Dr. Bowerbank—neither of them aware of the experiments which the other was pursuing—determined upon a close microscopical examination of the structural peculiarities of the bones, in the hope of eliciting some characters which would, in conjunction with their external forms, point out with some degree of certainty the class of animals to which these remains in reality belonged. They arrived independently at the same conclusion—that the bones of birds, reptiles, fishes, and mammals, each possess marked peculiarities which furnish a means of deciding disputed relations of obscure and difficult tribes of existing animals, as well as of ascertaining the true relations of such palæontological remains as it might be otherwise difficult or impossible, from their dilapidated condition, to refer to their real position amongst animals. In illustration of Dr. Strong's paper, Mr. Lee also exhibited

the head of a boy's thigh bone, which had come away from the patient after three years' suffering from scrofulous disease of the hip-joint; and a large portion of the shaft of the tibia of another boy, whose leg was injured by a kick whilst playing at football. In both cases a most successful cure had been effected under the surgical care of his friend, Dr. Wm. Price, of Margate.

Mr. Cushing exhibited a series of three selenite plates superposed on each other, and revolving at different speeds, capable of giving remarkably beautiful combinations of colour in conjunction with the polariscope, arranged by Mr. Becker; and read a carefully-composed paper "On the Polarization of Light, and the Methods of employing it in connection with the Microscope." The concluding paragraphs of it elicited some observations from the President on the value of having the polar axis of the selenite marked on the plate, as a means of determining the direction of tension of muscular fibre and other objects examined with it.

Mr. Cushing kindly presented to the library of the club a treatise "On Polarized Light," and "On the Use of the Oxy-hydrogen Microscope and Polariscope," by Mr. Chas. Woodward, F.R.S.

The thanks of the meeting having been given to Mr. Cushing,

The President next directed attention to Dr. Matthew's recently-invented turn-table, for making and varnishing cells, which, by an arrangement on the principle of the parallel rule, keeps the slide, whatever may be its diameter, perfectly concentric, and also admits of two cells being formed on the same slide if required. He also exhibited a pretty and portable microscope lamp, by Moginie, of 35, Queen Square, Holborn. It was packed in a tubular case, like that of a pocket folding telescope, and had the great advantage over a somewhat similarly fitted but much more expensive lamp, that, whereas the latter could only be carried in a vertical position without the oil escaping, Mr. Moginie had, by a simple contrivance, secured his lamp from leakage, even if turned bottom upwards.

Mr. F. Baldiston sent to the meeting some scorpions and an enormous spider from the West Indies, which had been received from Mr. John Gibson, of Canning Road, Addiscombe. The President made some interesting remarks on the stinging powers of the scorpion, and the nature and influence of its poison; and described some observations made by Mr. Frank Buckland and himself on the effect produced on a Gallago, by scorpion poison, and read a laughable account by Mr. Buckland of a fight between a scorpion and a mouse.

The following members exhibited sections of bone in illustration of Dr. Strong's paper, and other objects, with their microscopes:—Messrs. H. Lee, P. Crowley, T. Cushing, H. Long, C. Bonus, E. T. Jones, F. West, jun., C. W. Hovenden, J. D. Wood, H. McKean, and Dr. Strong.

Mr. Harry Townend, of Cheam, was elected a member of the club.

At the monthly meeting of this club, March 15th, Mr. J. S. Johnson, M.R.C.S., read a paper on "A Microscopical Examination of the Oyster." He went over the ground to be met with in most treatises on the subject of mollusca, and finally described various interesting

organisms which he had found on the shells of oysters, and exhibited several of them, such as *sertularia*, *foraminifera*, and *Serpula triquetra*, mounted for the microscope.

The President, Mr. H. Lee, having invited discussion amongst the members,

Mr. J. W. Flower said there was one singular feature about the oyster, namely, that in former days the species were much more abundant than at the present time, and that this was so had been abundantly proved by the enormous variety of fossil oysters found in the chalk and in the London clay.

Mr. Long said he had examined the liquor of oysters, and, by spontaneous evaporation, he had obtained some beautiful crystals.

Dr. Strong asked whether the body of the oyster, when the mantle was removed, was homogeneous.

The President said he did not believe that when once the young had escaped from between the valves of the parent they ever re-entered them; nor was it correct, as had been stated in a recently-issued official report, that they adhered *immediately*. This was the critical period of their existence. Oysters still bred and spawned just as freely as ever they did, but the young ones did not adhere. This was the cause of the scarcity of oysters. Why this took place no one could definitively say; but warmth and tranquillity were undoubtedly the great conditions of the prosperity of the young fry, and cold nights at the spatting season were fatal to them. At Herne Bay, in June, 1861, he found the whole water of the sea full of young oysters in their swimming condition; but during that month the nights were very cold, and there was no fall of spat that year. That a low temperature was unfavourable to the development of the young oysters had been proved by an experiment made by Mr. Buckland and himself. Some of them were placed in sea water in a glass jar, and whilst they were swimming vigorously about, a piece of ice was dropped into the vessel. The immediate effect was that they all sank to the bottom, apparently exhausted; but on the withdrawal of the ice, and restoration of the former temperature, they revived. He agreed with Mr. Flower that all the British oysters were varieties of one species, and had satisfied himself that the formation of their shell depended on the conditions and necessities of their habitat, and on the chemical constituents of the water, mud, &c., in which they existed. He placed some Swansea oysters in the experimental ponds, owned by Mr. Buckland and himself, at Reculvers. The shells of the oysters, from the former locality, were particularly heavy and clumpy; but the new growth put on all the characteristics of the true "native." They had also had American oysters in their beds, but the growth of the shells had not been sufficiently marked to be conclusive, and, in fact, these American oysters were totally different in character and form from the British oyster.

A cordial vote of thanks was given to Mr. Johnson for his lecture; which had given rise to an interesting discussion.

The President announced that since the last meeting three new Microscopical clubs had been formed; namely, "The Sydenham and

Forest-hill Microscopical Club;" "The Margate Microscopical Club," at the inauguration of which he had assisted on the previous Monday evening; and "The South London Microscopical and Natural History Club," a preliminary meeting of which he had also attended on the 11th inst. This latter society would especially direct its attention to the Natural History of the county of Surrey. He was sure that the members of the Croydon Club would be glad to fraternize with all these newly-formed societies; and he had no doubt arrangements might be made for their co-operation with each other in systematic work, and for the members of all of them joining in excursions under the leadership of the most experienced men belonging to each.

The following presents had been received;—The 'Transactions of the Quekett Microscopical Club,' from that club. Davies's work 'On the Preparation and Mounting of Microscopic Objects,' from the President. Johann' Nave 'On the Collection and Mounting of Algæ, &c.,' from the President.

Messrs. W. H. Beeby, J. S. Crowley, and W. T. Loy, were elected members of the club.

The following members exhibited objects with their microscopes:—Messrs. H. Long, J. W. Flower, F. W. Gill, J. T. Johnson, K. McKean, F. West, jun., G. Manners, J. D. Wood, H. Long, and C. W. Hovenden.

Brighton and Sussex Natural History Society.

April 13th. Ordinary Meeting. Mr. T. H. Hennah, Vice-President, in the chair.—Messrs. W. Jackson, W. H. Ross, and H. Saunders were elected ordinary members.

Mr. Wonfor announced the receipt of the 7th Annual Report of the Lewes Natural History Society, and the April number of the 'Quekett Club Journal,' from the secretaries, and "Mitten's South American Mosses" ('Linnean Society's Journal') from Dr. Addison. Votes of thanks to be given to the donors.

An interesting communication from Dr. Stevens, of St. Mary Bourne, "On certain Types of Flint Implements found in Hampshire," was read, and a vote of thanks passed to Dr. Stevens.

Mr. Wonfor then read a paper "On What is Coal?" in which, among other interesting matter, he pointed out that the true vegetable origin of coal was not only determined by observing the conditions under which it occurred, but by the fossil remains associated with it, and by the results of microscopic examination. These showed that coal was simply vegetable matter, altered and compressed.

The vegetation which helped to form coal was characterized by an almost absence of that kind of wood now found in our forest trees, and by a preponderance of ferns, calamites, and club-mosses, very few of which retained their form sufficiently to admit of a satisfactory demonstration of what they were really like, though fern fronds, more or less mutilated, detached roots and stems, occasional cones, fruits, fragments of flowers and fructification helped to determine some of the orders of plants.

The researches of Professors Morris and Huxley, Mr. Carruthers, and Dr. Dawson of Canada, pointed to the fact that the great bulk of the bituminous coal consisted of sporangia and spores of plants allied to our existing club-mosses, while thin sections of coal which he would show at the Microscopical Meeting revealed the fact that the chief elements in their composition were these said spores and sporecases, the latter about $\frac{1}{23}$rd of an inch in diameter, somewhat resembling bags or sacs, more or less flattened, and containing the former, irregularly-rounded bodies about $\frac{1}{700}$th of an inch in diameter.

The processes by which coal was supposed to have been found from vegetable matter, and many other interesting points, were discussed, and the paper illustrated by specimens and fossils from different coal-fields.

A vote of thanks was given to Mr. Wonfor.

April 27th.—Microscopical Meeting. Mr. T. H. Hennah in the chair.

Mr. R. Glaisyer announced the receipt of two slides, one a section of the *morel*, for the cabinet, from Mr. Wonfor.

Mr. Hennah read a very interesting communication from Dr. Addison "On the Water Flea" (*Daphnia pulex*), containing original observations on the moulting of the carapace of the female and the birth of young Daphnia from agamic eggs, from which it would appear that the two acts are simultaneous.

Mr. Marshall Hall exhibited a new pocket-lamp by Moginie, of London, which appeared to be a very compact and portable apparatus.

Mr. Wonfor exhibited a fresh specimen of the morel (*Morchelia esculenta*) obtained near Brighton.

The meeting then became a conversazione, when

Mr. Marshall Hall exhibited spicules of the new sponge (*Pheronema Grayii*), dredged up by him off the coast of Spain.

Mr. Sewell exhibited scalariform tissue of fern, sections of cocoanut wood, whalebone, &c.

Mr. Hennah, under one of Beck's new $\frac{1}{10}$th immersion lenses, exhibited living diatoms. The performance of this lens was pronounced perfect, the definition being very precise, while the distance at which it worked was an ordinary live-box cover. The same objects were also shown with a Gundlach's $\frac{1}{16}$th, which gave very good definition.

Mr. Wonfor exhibited sections of the morel, showing the spores in their receptacles; sections of coal fossils, by Norman, of City Road, London, in which leaves, roots, and stems of Lepidodendra, &c., were well seen; sections of coal made by Mr. Slade, and described in the January part of the 'Quekett Club Journal,' and kindly lent for the occasion; and a series of sections of coal and lignite made by himself, including Torbane hill, white coal of Tasmania, brown coal of Bohemia, the Bradford Belter Bed, &c., in which not only woody fibre, but also spores and sporangia, were distinctly made out; these were in illustration of his paper "On Coal," read at the last Ordinary Meeting.

In the course of the evening Mr. Wonfor illustrated the method

by which he had made and mounted thin sections of coal, which was a modification of the different published methods.

It was announced that the next Microscopical Meeting, being the Anniversary Meeting, would be a General Evening and conversazione.

It was also announced that the first Field Excursion would take place on Saturday, May 6th, to Balcombe for Tilgate Forest.

May 11th.—Ordinary Meeting. Mr. T. H. Hennah in the chair.

Mr. T. M. Fowler was elected an ordinary member.

It was resolved that a letter of condolence be written to Mrs. Peek on the death of her husband, formerly a member of the Society.

Mr. Wonfor reported on the success of the Field Excursion of May 6th to Balcombe, and gave an account of some of the objects seen and obtained.

The Rev. J. H. Cross exhibited and presented for the Society's album, sketches he had made on the occasion.

Mr. J. Dennant exhibited a series of marbles from the Pyrenees, and large acorn cups of the Smyrna oak, *Quercus ægiolops*, the Valonia of commerce.

Mr. J. Howell exhibited fossil Silurian corals, vertebra of Plesiosaurus, from the bone bed of Aust; and shells from the lias at Bristol, and the Upper Eocene, Isle of Wight.

Mr. Saunders exhibited red organ coral and other recent corals; pottery from the tombs at Bengazi and Pompeii; and sand from the Bays of Valentia, Malta, and Taranaki.

Mr. Elphick exhibited a couple of piebald mice, taken from a rick, and believed to be a cross between the common and white mice.

Mr. Wonfor exhibited a specimen of the silicious sponge *Euplectella mirabilis;* specimens of yellow wagtail; grey-headed ditto; and a state either intermediate between the two or in the immature plumage of the second. While the first is common, the second is uncommon, and the last very rare. These birds were kindly lent by Messrs. Pratt and Sons, and had been obtained in the neighbourhood of Brighton; also a very dark variety of the northern oak eggar moth, *Bombyx callune*. It was resolved that the next Field Excursion be on Saturday, June 3rd, to Barcombe for Plashett Wood.

May 25th.—Microscopical Meeting. Mr. M. Penley, Vice-President, in the chair.

Being the Anniversary Microscopical Meeting, Mr. R. Glaisyer reported that 122 slides and a Möller's diatom type slide had been added to the cabinet during the year.

Mr. Wonfor, Hon. Sec., gave a brief abstract of the 'Proceedings,' with an account of the papers read and the work done, which, he said, had exceeded their anticipations, when it was determined twelve months since to hold monthly microscopical meetings. Judging from what they had done in advancing microscopical inquiry among the members during the past year, augured well for increased exertions in the forthcoming year.

Mr. Wonfor then gave an account of a method for obtaining thin sections of soft rocks, and illustrated it by making and mounting

sections. He first cut with a saw (the one used was the common fret saw) slices of oolite, &c., ground down one surface on glass-paper of different degrees of fineness, and fastened them by the ground surface with moderately stiff heated Canada balsam to glass slides. As soon as cold the other surface was ground down to as thin a degree as required, always finishing on *very* fine glass-paper. The superfluous balsam was then cleared away and the powdery matter cleaned off with spirits of wine, when the slide was ready for the cabinet, or could be covered with thin cold balsam and a glass cover and left to harden. The specimens of oolite, which were used to illustrate, were part of the stone employed in making the Brighton aquarium. He had at present only worked on different oolites, Portland stone, dolomite, and nummulites from Egypt. By the same process he had made very thin sections of coal—in fact, the examples of coal shown at the last meeting were made by this process. It was at the suggestion of Mr. Marshall Hall, who asked him to try how it would act on oolitic limestone and dolomite, that he was led to attempt the process. In the case of Portland stone, he had found it advisable to rub it down roughly first on a piece of paving stone, and to finish it off on glass-paper. Some sections he had cut, ground, and finished for the cabinet in twenty minutes.

The meeting then became a conversazione, when

Mr. J. Dennant exhibited deep-sea Atlantic soundings, fossil and recent diatoms, antennæ of drinker moth, &c.

Mr. T. Cooper exhibited crystals of hematoxylin, tartrate of soda, and other salts.

Mr. R. Glaisyer exhibited sections of *Eozoon Canadense*, Purbeck and encrinital limestones.

Mr. Turner exhibited sections of Indian rice-paper, root of *Osmunda regalis*, and spores of morel.

Mr. Wonfor exhibited sections of different oolites, dolomites, Portland stone, nummulites from Ben Hassan, and crystals of salicine in silicate of soda, and crystals of silicate of soda, mounted in the same. These latter formed very beautiful polariscope objects, and, with one of Ackland's selenite stages, gave a wonderful variety of colour.

It was announced that the subject for the next Microscopical Meeting on June 22nd would be "Vegetable Hairs and Scales."

State Microscopical Society of Illinois.

On the evening of March 17th the State Microscopical Society of Illinois celebrated its third annual réunion by a magnificent exhibition in Farwell Hall. The main floor was crowded with a large and delightful audience, who moved in steadily-recurring streams from table to table. There were about 100 instruments on exhibition, and at least 1000 slides, though there was neither time nor opportunity to make all of them available for use.

Such has been the accomplished labour of the Microscopical Society, a labour whose greatness can be more readily appreciated when it is recollected that the Society was only organized about three years

since. At its first conversazione, held at the house of Mr. Joseph T. Ryerson, about thirty instruments were exhibited, estimated to have cost, in connection with their appurtenances, about $7000. On the date of its third conversazione, more than 100 instruments were exhibited, representing a value of $30,000. Such has been the growth of our Microscopical Society, of which it may be said that it stands the first of its kind in the United States.

While the slides were being changed on the instruments, Prof. H. Peabody performed interesting electrical experiments, and Prof. Delafontaine and Mr. Boerlin showed several beautiful Geissler tubes lit by the electrical current.

The Tunbridge Wells Microscopical Society.*

The monthly meetings of this Society have been held regularly under the able presidency of Dr. Deakin.

The subjects for discussion have been Sections, Vegetable and Animal, the careful consideration of which has occupied the members for several meetings, and elicited much useful information.

The next meeting will take place on October 3.

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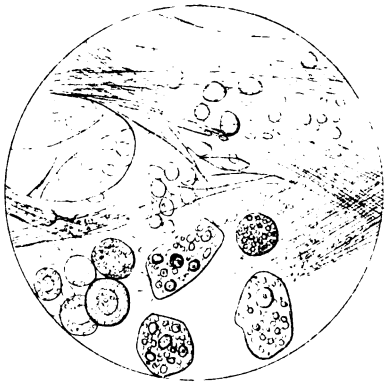
* From the Rev. Mr. Whitelock.

1



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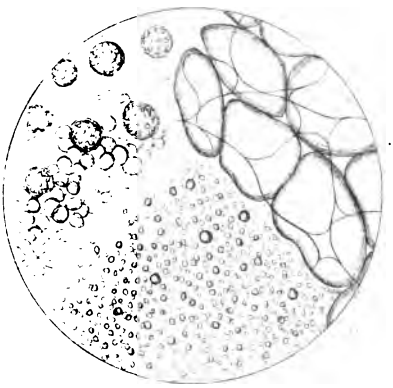

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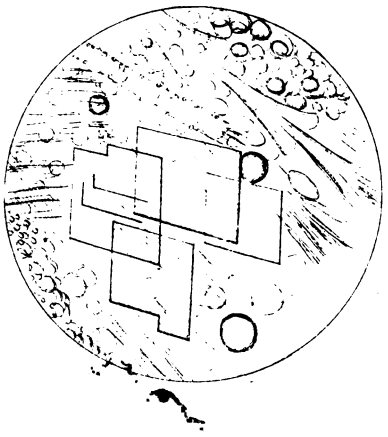

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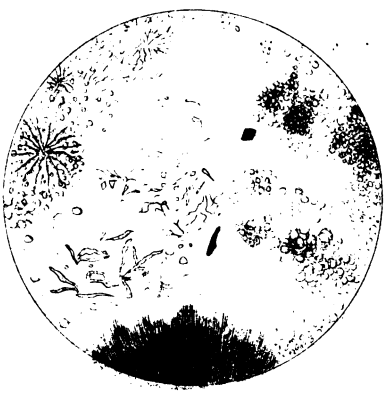

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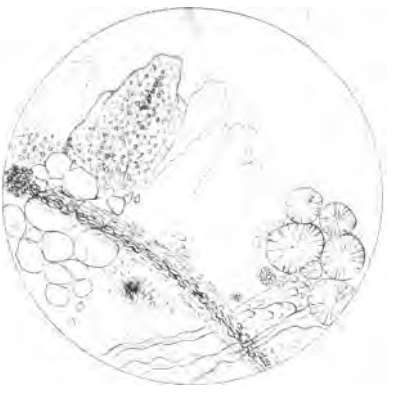

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THE

MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1871.

I.—*Mycetoma: the Madura or Fungus-foot of India.*

By JABEZ HOGG, Hon. Sec. R.M.S., Surgeon to the Royal Westminster Ophthalmic Hospital, &c., &c.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *June* 7, 1871.)

PLATE XCII.

A FEW years ago Dr. Vandyke Carter, of the Bombay army, made us acquainted with a certain specific form of microscopic fungus, which he alleges produces, among the native inhabitants of Madura, and certain other districts in India, a peculiar disease, and since recognized as the *Fungus-foot of India* (Mycetoma). A number of specimens of the foot have been examined in this country, and it is thought by some histologists and pathologists that most of them exhibit the ravages of a fungus. It appears, however, that now and then specimens have failed to satisfy those into whose hands they have fallen of the fungoid character of the disease; Dr. Carter speaks of such specimens as a variety, and in place of a living fungus he says "numerous rounded bodies of a structureless or finely granular appearance are seen, in which the fungus particles were free from crystalline fringe, but still showing a cellular structure, the true nature of which is *degenerate fungi*."* A

DESCRIPTION OF PLATE XCII.

No. 1.—Altered fibrous tissue, mulberry-shaped fat-cells, &c., × 150.

„ 2.—Fat-vesicle and molecular matter, chiefly fat; a few mother-cells filled with fat-granules, and granular contents of others distributed over field. × 150.

„ 3.—Crystalline matter (stearine? &c.), fat-globules; vegetable hairs, &c. × 150.

„ 4.—Algoid filaments in matrix or stroma with fatty molecules and pigment granules, × 150. This specimen was taken from the second or more frequently recurring form of diseased foot.

„ 5.—Portion of a foot bone, the compact tissue and lamellæ of which have been removed, and the cancellous structure occupied by blacked masses of inorganic matter, × 50.

„ 6.—Papillæ hypertrophied, and filled with granular matter; remains of a capillary seen running over the field; connective tissue and fat-corpuscles filled with crystalline particles. × 150.

* 'Trans. Bombay Medical Soc.,' 1860, '61, and '62; 'Medico-Chir. Review,' vol. i., 1863.

specimen of this "variety" appears to have perplexed Dr. Ballingall, as well as the late eminent microscopist, Professor Quekett, both of whom were in consequence unable to pronounce the disease to be fungoid; due to the growth and ravages of a fungus. At the end of the year 1869, a foot of the doubtful kind was placed for examination in my hands, and those of a well-known excellent pathologist and histologist: I was requested to report on the specimen. You may imagine, therefore, I was most anxious that everything should be conducted with the care and caution which so responsible a position would naturally inspire. I will tell you at once I was not a little surprised and disappointed to be obliged to come to the conclusion that no trace of a fungus could be found in any part of the foot; I say disappointed, because from what I had read about the fungus-foot disease of India, I expected no difficulty whatever in the matter, and I will add more by way of caution, that the first few sections which were made and washed in distilled water, for the purpose of freeing them from some apparently crystalline and fatty matters, showed both the spores and mycelium of a fungus. Here then I thought there could be no mistake, and put the specimens away to further examine at a more convenient opportunity. The next day I resumed my work, and made other sections from the foot, which I at once transferred to a weak solution of spirit and glycerine. On examining them I was not a little puzzled to find abundance of fatty matters, but not a particle of anything like a fungus. Subsequent examinations convinced me an error had occurred somewhere in my first observations, and I then examined the distilled water. A single dip from the bottle gave me a plentiful crop of fungus, exactly resembling that found in my first specimens. On taking up a well-corked bottle of arsenical solution standing near the distilled water, I saw it contained numerous tufts of a fungus, which, as you know, abound everywhere, and spread with amazing rapidity upon almost everything, ripening and depositing their spores, with powers of self-increase so rapid as to be almost incredible. The naked-eye appearances of this fungus-foot may thus be briefly stated. The foot was greatly enlarged and swollen; all fair outline being lost. There were numerous excrescences or raised bodies over the upper surface; none on the lower; which at first sight might be supposed to communicate with the internal parts; but on attempting to pass a small silver probe through the centre of any one of them, it could not be made to penetrate more than a very short distance, and I doubt very much whether there could have been any actual sinus leading to the bone at any time. There might, however, have been an ulcerating sore during life, which the hardening nature of the methylated spirit, in which the specimen was preserved, had entirely obliterated. On making a vertical section of

the foot, so much confusion of parts existed, that the muscular, fibrous, and other tissues seemed to be blended in a gelatinous mass; on removing portions of the bony mass, the cancellated interspaces, which were much larger than usual, were occupied by numerous whitish granular bodies, somewhat resembling millet seeds. These bodies, which are described by Dr. Carter as pink in the fresh foot, were apparently mixed up with a crystalline material. But fatty matter so predominated that it was almost impossible to free any section from it, without resorting to boiling in æther, or liquor potassæ. When boiling in the latter was continued for a few minutes, nearly the whole was held in solution, the residue being a very small quantity of fibrous tissue. Even fragments of bone almost disappeared when treated in the same way; whereas, if the spores and mycelium of a fungus are subjected to the same process, the fragments that remain enable us to recognize them without difficulty. Fungi resist the action of boiling fluids as they do prolonged and intense cold, so that we need be under no apprehension of losing all trace of them, if they ever had an existence, while subjecting animal matters to the crucial test of boiling in destructive reagents. A prolonged and exceedingly careful microscopical examination yielded only negative results, so far as fungi were concerned. The cells and fibres which Dr. Carter says "are imbedded in black masses of matter," could nowhere be traced; neither could I see "the fish-roe-like substances made up of defaced fungus structure." The little rounded bodies in this specimen were not uniform in structure, and were mostly imbedded in a gelatinous fatty matter in the interspaces of the bones. The pigment of the skin, generally so abundant in the black races, was entirely removed; while the papillæ were so much hypertrophied, swollen up, that all trace of ordinary structure seemed lost, not even a perfect epithelial cell remaining. The extraordinary way in which the pigment had disappeared induced me to think that even "black fungus masses" might owe much of their colour to disintegrated pigment granules, and even take up new forms in the interspaces of the metamorphosed muscular and fibrous tissues. Portions of the subfilamentous material presented, at first sight, an appearance somewhat resembling ciliated epithelium; these masses easily separated and floated about, and there was no nucleus seen, and only a slight fibrillation. Fat abounded and was often arranged and massed in cells, in which were groups of smaller corpuscles, in some instances presenting a false appearance of nucleated cells. The subcutaneous infiltration of fatty matter, and the disintegration of nervous matter, muscular and other tissues occasioned thereby, gave to all the specimens examined a confused resemblance; and although some few bodies of "a spindle shape" were seen, it would require a considerable stretch of imagination to believe that

they were either "ciliated epithelium," "degenerate fungi," or the altered forms of "a true oidium," the material contents of "the branching tubular canals" of which had become altered through some kind of natural quiescence or encystment. If it be possible for such encystment to take place, it must, in my opinion, be a complete disguise of all known fungus characteristics, and under such a disguise it was not at all surprising the late Professor Quekett should fail to come to "any definite opinion of its character." I would not have you suppose that a doubt exists in my mind about the finding of fungi by Dr. Carter, in connection with the remarkable form of disease with which he has made us acquainted. I have placed a section under a microscope, taken from a recent specimen sent over to this country, and now in the possession of Dr. Tilbury Fox. It belongs to the more frequently occurring form of disease, and in which are seen many black, or deep-brown coloured masses, either closely aggregated, or having a radiating aspect, branching out in every direction;* and what to me seems very curious, several spore-like bodies closely resembling "Puccinia" have been found; which being a vegetable feeder, should not, according to the Rev. Mr. Berkeley, occur among animal matter. On the same authority we are assured "there is not the slightest ground for supposing that the disease depends on inoculation with the spores of the true parasitic fungi belonging to the rusts and mildews."† Nevertheless I believe such spores, as well as the conidial form of oidium, have been found in a few of the specimens; these may, however, have been accidentally introduced from without. Dr. Carter does not tell us whether the less frequently observed variety of diseased foot, that is, the foot in which "degenerate fungi" and numerous rounded bodies are seen to be the chief elements of destruction, is a more advanced, has existed longer, or is a worse stage of disease. It ought to be so if the fungi are in a more advanced condition; but it certainly is not, that is, if ordinary appearances can be accepted as any guide to a conclusion on such a point. I can hardly believe, however, that what he describes as "degenerate fungi" are fungi at all. I am ready to admit, however, so much of Mr. Berkeley's argument, that at times "they so nearly simulate fungus growths, that it is difficult to get rid of the notion that they are really vegetable growths." But if they were, I see no proof anywhere adduced to show that the diseased condition, described as due to a fungoid growth, is really so; and this is the important point, one which

* Since my paper was read, my friend Mr. Bell has made a chemical analysis of these "black masses," and finds them to consist "of fatty matter, phosphates of iron and lime, a little carbonate of lime, and a minute quantity of an organic substance, albumen or fibrine."

† Rev. M. J. Berkeley "On the Fungus-foot of India," 'Intellectual Observer,' vol. ii., 1863, p. 248.

should, if possible, be cleared up. Are not the algoid filaments another instance of a vegetable growth rapidly developed after death in a putrescible substance? It is a matter of some moment in a scientific point of view, that this question should be carefully investigated and answered, for I find the eminent Mycologist already referred to accepting Dr. Carter's hypothesis as a demonstrated fact, describing the fungus as a *new species*, and assigning to it a name; remarking, at the same time, that "although the fungus resembles closely the genus Mucor, there is no columella in the sporangium, a character which accords with Chionyphe rather than Mucor;" nevertheless he places it with the latter, while he admits that Chionyphe is one of those species *only found under snow*. He concludes with what I should regard as a bit of special pleading for a pet hypothesis, because you must remember while we are discussing the action of a fungus in a living animal, Berkeley refers solely to its action on dead matter; and whatever that action may be, there can be no similarity in the two processes. "It is," he observes, "highly probable that many of our common moulds commence with a similar condition. The first indication of a change in tainted meat, is seen to commence with little gelatinous spots of vegetation of various colours, the early stage of some curious species of Aspergillus, or Penicillium."* Hospital gangrene may, he thinks, also depend upon a similar cause. I think, neither Mr. Berkeley nor anyone else can bring forward a particle of proof in support of such a probability, and which is after all no nearer the truth than the many guesses that have been made at a germ-theory of disease generally. To establish Dr. Carter's fungoid origin of disease, it is absolutely necessary to show that the spores of a vegetable fungus can get into the dense structures of the animal body during life, there germinate, and destroy the hard bony tissues, and ultimately kill the patient. At a glance, the character of the tissues might seem to make this impossible; and Mr. Berkeley evidently has his misgivings on the point, for he writes, "the little granular bodies are so closely involved in stearine, that their germination is scarcely probable." If we next take the symptoms and appearances which usher in the disease as described by Dr. Carter, we shall see how far it may become possible for fungi to pass in through the sinus openings. "The foot swells up, is of a dark colour, numerous sinuses appear, with pink stains or streaks, which penetrate the subjacent tissues, and end in spherical groups of bright orange-coloured particles. The sinuses are more or less lengthy and tortuous, and will not

* In the 'Intellectual Observer' we are favoured with a somewhat remarkable series of illustrations, of some very curious matters. A whole page is given, resembling nothing in the shape of fungi, but rather what I should regard as extraneous vegetable particles in a specimen.

usually yield to *pressure of the probe*," &c. Nevertheless we are expected to see that the soft, yielding spores of a fungus will find their way through these tortuous sinuses, passing along in an opposite direction to a strong outward flow of a sanious discharge, which usually accompanies such a condition of disease. Again, the existence of a sinus presupposes a grave state of disease. Dr. Carter does not for a moment believe that the sporules, although minute enough, could possibly enter through the circulation. A more generally expressed opinion, and an equally probable mode of conveying the contagium to the internal parts of the body, the endemic character of the disease, would, in this way, be more easily accounted for in districts where the growing crops of rice were at one time seen to be devastated by "smut," and thought to be the cause of the cholera visitation. But it could not be believed to enter through the blood, because in such a case it would be impossible to understand why the spores of a fungus should select a hand or a foot, and find in them a more congenial soil than in other parts of the human frame; why one foot should be destroyed and the other escape; or why the poison should stop at the part where the bones of the leg join the foot, and so forth.

The constant occurrence in the internal organs of algoid growths has long been noticed—Sarcina, for instance, in the stomach and bladder; but after the disease has existed for years, it has not been observed to destroy life; indeed it often produces so little disturbance, that it is only detected after death. The other so-called fungoid diseases, such as those which some believe to be the cause of gangrene, cholera, &c., I need not dwell upon, because they rest their claims to consideration upon the most inconclusive of experiments and observations.

The incubation of the disease demands a passing notice; as, according to Dr. Carter, it more frequently affects the agricultural classes, men in the vigour of life; is not associated with any constitutional causes, and is not known to be transmitted. But as agricultural labourers go about barefooted, and seldom wash their feet thoroughly, it therefore happens that the spores of a fungus penetrate the hardened skin, and produce "worse ravages than the dreaded guinea-worm." I must confess I do not understand this peculiar line of argument; for although I can easily see how the guinea-worm makes its way through the skin, particularly if softened by standing in water, I cannot see how the spores of a fungus should be capable of exerting the same force as an animal parasite provided with a mouth and jaws, and a strong desire to provide a comfortable lodging in the leg or foot of the first animal that comes in its way.

It must be admitted, if the disease originates in a fungoid growth there should be no instance of a foot which does not

bear evidences of the characteristic poison. Such a specimen as I have been discussing, without a particle of fungus, is enough to invalidate and destroy the superstructure upon which Dr. Carter builds his hypothesis of the "*fungoid foot*"; and my objection is in no wise met by saying that this form of disease is exceptional, and the appearances observed are those of "degenerate fungi," &c. To this I reply, it is apparently a form often met with. Mr. Henry J. Carter, F.R.S., in his early investigations of the disease, found only a large quantity of albuminoid and fatty matters, and attributed the changes observed to fatty degeneration. He, however, subsequently examined other specimens in which he discovered fungi, and changed his opinion, but he adds, "I could scarcely overcome the difficulty in believing it possible for a fungus to destroy the bones as well as the other tissues of the foot." Another excellent observer, Dr. Bristowe, a gentleman who has examined several specimens of the disease, writes:—"Although the soft parts are infiltrated with a lump of *truffle-like bodies*, I am not prepared to say that the fungus causes the disease; it rather seems to me probable that the primary disease was caries of the bones, and that the fungus became developed subsequently and accidentally. The latter view is supported by the nature of the foot which you examined." I feel bound to believe with Dr. Bristowe that the disease is due to caries of the bones; occurring, perhaps, in a strumous, scrofulous, or syphilitic constitution. In caries, we find a similar train of pathological appearances; the bony structures are filled with a sanious, or glary fluid, soft granulations springing up, and a deposit of tuberculous material, with an increase of fat, causes complete destruction of the bones. The slow disintegration of the various structures in the Madura foot disease is exaggerated by the ordinary effects of a tropical climate, often an important factor in disease, and one well exemplified in those remarkable forms elephantiasis and leprosy, both of which seem to originate in a metamorphosis of cell contents, a condition not unfrequently noticed in pathological anatomy. The deposition of fat in cells and structures of all kinds is perhaps of all changes the most curious and universal. A beautiful series of transformations is often traced in fat-cells, which, according to the deficiency or excess of nutritive fluid, lose, in the former case their contents, and eventually contain only serum, in the latter become distended with fat-globules; further, in the cells of glands secreting fat, which, at first poor in fat, are ultimately quite distended with it. Also in the ova of all animals which deposit fat and proteine within themselves.

In the case before us of the Madura foot, the fatty degeneration, or disintegration, commences in the bones of the foot, and physiological phenomena are gradually merged into pathological. A similar instance is presented in the amyloid "*Lardaceous*" disease,

which invades various parts of the body. Large quantities of a fatty material accumulate from a supposed deficiency in the quantity of alkali in the blood. Complexity of structure in the known characters of organic compounds seems to be never better exemplified than it is when large quantities of fatty matters enter into nitrogenous compounds. As an example, the decompositions effected by butyric acid seem to be endless, and more especially so when connective and fibrous tissues enter into these decompositions, and give new shapes and characters to the organic molecules. These again are immensely changed, and other transformations effected by the putrefactive process. Animal matter in a state of putrefaction acts as a ferment, rapidly changing albuminoid and fatty particles into a fungus, and is capable of causing their metamorphosis into sugar, alcohol, and carbonic acid. It may be possible that an allied process is going on in connection with the Madura foot disease, a putrefactive ferment, a process of chemical disintegration while the limb is still in connection with a living body, although itself dead. I have before ventured to affirm that parasitic fungi are characterized throughout nature by feeding on effete or decayed matters, and I see no reason for changing my opinion. This view of them seems to have been floating in the mind of the Rev. Mr. Berkeley, for he concluded the paper I have already referred to, by observing:—"In some cases it would seem as if the foot was already in a diseased state when the fungus was introduced: at least the history of one case which apparently commenced with a boil in the instep, and opened by a thorn, indicates such a lesion as might well encourage the growth of a fatal parasite."

I readily admit that Dr. Carter's great experience of Mycetoma, and the many opportunities he has had of making examinations soon after amputation of the foot, entitles his opinion to great weight. I should indeed have been much inclined to accept his views from this circumstance, provided he could have furnished indisputable or reasonable evidence that "Mycetoma stands for a form of swelling which is caused by the growth of a fungus." I have endeavoured to place the matter before you in an impartial spirit, hoping thereby to assist in the elucidation of an important question, at the same time trusting I may have made the subject sufficiently interesting to induce the Fellows of this Society to investigate it for themselves: if they do I can promise this much, that they will find it of far more importance, if not more interesting, than the markings on Diatoms and Podura scales.

II.—*Diatomaceous Earth from the Lake of Valencia, Caracas.*

By A. Ernst, Esq., and H. J. Slack, F.G.S.

(*Read before the* Royal Microscopical Society, *June* 7, 1871.)

The following letter accompanied a present to the Society of diatomaceous earth from the Lake of Valencia, Caracas:—

To the Secretary of the Royal Microscopical Society, London.

Caracas (Venezuela), *Dec. 4th*, 1870.

Sir,—Allow me to present to the Royal Microscopical Society the included specimen of *diatomaceous earth* from the neighbourhood of the Lake of Valencia in this country. This lake is known for the progressive diminution of its waters, and was formerly very much larger than it is at present. An interesting account of it is given by Humboldt, 'Personal Narrative,' ii., 1–20 (Bohn's edition). All round the lake, to a very considerable distance, extends a deposit called by the inhabitants *tierra de caracolillo*, i. e. *earth* (formed) *of small shells*, undoubtedly the old bottom of the lake. It forms a layer of several feet of thickness, in which there are imbedded numberless specimens of small shells. I have hitherto found six different species, of which I add samples. Nos. 1–4 are very abundant; 5 and 6 are very rare. I hope one of your conchologists will have the kindness to determine the species. The remainder of the layer has the appearance of a greyish blue marl, and contains so great a number of fossil diatomaceæ that it may be said to be entirely formed out of them. Being myself not sufficiently acquainted with diatomaceæ, I must leave the closer inspection of this earth to others, and I feel sure that one of the members of the Royal Microscopical Society will kindly give us a list of the species it contains.

I shall be very happy to send a sample of the *tierra de caracolillo* to any microscopist or geologist who would like to have some.

I have the honour to be, Sir,

Your obedient servant,

A. Ernst.

Humboldt visited the Lake of Valencia about the beginning of the present century. He describes the lake as then resembling that of Geneva in appearance, and that of Neufchatel in size, and makes many remarks on its progressive diminution, which he ascribed to local changes, increasing evaporation, and diminishing the water supplies. He remarked, "The land that surrounds the Lake of Valencia being entirely flat and even, what I daily observed in the lakes of Mexico takes place here; a diminution of a few inches in the level of the water exposes a vast extent of ground covered with fertile mud and organic remains."* He advised the

* 'Personal Narrative,' Helen Maria Williams' trans., vol. iv., p. 151.

rich landholders to place columns of granite in the basin of the lake, and note the mean height of the waters from year to year. At the time of his visit the mean depth of the lake was from twelve to fifteen fathoms, and in the deepest parts thirty-five or forty feet. He said, "It is impossible to anticipate the limits, more or less narrow, to which this basin of water will one day be confined, when an equilibrium between the streams flowing in and the produce of evaporation and filtration shall be completely established. The idea, very generally spread, that the lake will soon entirely disappear, seems to me chimerical. If in consequence of great earthquakes, or some other causes equally mysterious, ten very humid years should succeed to long drought; if the mountains should clothe themselves anew with forests, and great trees overshadow the shore and plains of Aragua, we should probably see the volume of the waters augment and menace the beautiful cultivation which now branches on the basin of the lake."

The diatomaceous deposit of which Mr. Ernst has sent a specimen is remarkably rich in quantity of specimens; and it will interest microscopists to note some of the conditions, described in the preceding quotations, under which it has taken place.

A shallow lake, fed by numerous small rivers, having no sea outlet, and a warm temperature such as that of Valencia, seems very favourable to diatom life, and the preservation of the shells. Humboldt thought much of the water in the interior of Australia was in a similar condition; and friends of science who may have opportunities of visiting such localities will do well to follow Mr. Ernst's example, and forward specimens to the Society.

The fresh-water shells sent by Mr. Ernst have been named by Mr. Henry Woodward, and consist of the following species, the numbers being those on the little bottles containing them:—1. Planorbis; 2. Paludina (pygmæa?); 3. Bithynia; 4. Melania; 5. Physa; 6. Ancylus.

The diatoms, so far as they have been examined, do not seem to present any unusual forms. They will be still further investigated by Fellows of the Society who devote attention to this particular subject.

III.—*The Silicious Deposit in Pinnulariæ.*

By Henry J. Slack, F.G.S., Sec. R.M.S.

(*Received during the Recess, and taken as read.*)

In making Max Schultze's artificial diatoms, all sorts of patterns and gradations of size of the spherules are obtained, as the writer has called attention to in a former paper. This, with other facts, suggests the probability that in natural diatoms the silica may always be deposited in spherules, and that what have appeared plane surfaces, have the same structure as dotted ones, but on too minute a scale to be discerned with the means employed.

The uniformity of plan in the silicious deposits of diatoms is to a great extent shown, and to a still further extent suggested by examining one of Möller's admirable "type slides," with a good immersion $\frac{1}{8}$th or higher power. The gradations from large beads distinctly separated, to smaller beads closely approximating, are readily and instructively exhibited, so that it is easy to trace a series, beginning with large forms that present no difficulty of resolution, and concluding with the most delicate that tax the utmost power of the optical apparatus. When beading appears minute under high magnification, and each bead seems in contact with its neighbour: the outline of a section made by a plane passing through the bead rows perpendicular to the uppermost point of their circumference, would exhibit a delicate wavy line, the depressions of which would be extremely small, as they would correspond with the radii of the little spheres, while the width of the curves would correspond with their diameters. All that can be done under these circumstances, by the best adjustments, and the most careful unilateral illumination, is to exhibit minute, and often very faint, alternations of light and shade, indicating rather than demonstratively showing the character of the structure. When the best has been done with any objective, it becomes evident that a slight increase of the difficulty, from greater minuteness of the structure, would render it invisible, and make the surface look plane.

From these considerations it would be evident that if we can trace the spherule structure on similar parts of a series of diatom valves, and view the spherules in various gradations, from comparatively large sizes to the smallest our glasses will show, we are justified in supposing, if not in assuming, that similar parts of diatoms in which no structure at all can be made out, really possess the same structure as the preceding, though it eludes our view.

An examination of a number of species belonging to the genus Pinnularia has confirmed this view. At one time it was supposed that the Pinnulariæ were distinguished from allied forms by solid costæ replacing beaded bands. More recently this distinction has

not been deemed valid, but the writer is not sure that it has been overthrown as respects many species.

An examination of the Pinnulariæ on Möller's "type slide," led to the belief that the costæ of such diatoms as *P. viridis, nobilis*, &c., had been misunderstood. That instead of broad irresolvable ribs, a truer view exhibited fine lines of beads springing from the median band, with a furrow between them, and in that furrow another line of beads at a lower level. The curvature of these valves not only renders the exhibition of this structure very difficult, but presents it under perspective aspects by no means easy to understand. Occasionally a valve is met with that proves comparatively easy, and then no one would doubt the complexity of the so-called costæ, though much speculation would arise as to the most correct interpretation of the various perspective views that can be obtained.

The general aspect of valves whose costæ can be resolved is that of a number of lines or rows of beads springing from the median band in a series of narrow arches with long sides, in shape something like ladies' hair-pins, with hollows between them, in which one or more rows of beads may be discerned.

To investigate these appearances and the various gradations in different species was found impracticable with solids obtainable in London, and accordingly Herr Möller was applied to through Mr. Baker, and a considerable series, mounted dry, was lately received, and forms the subject of the following remarks:—

PINNULARIA INTERRUPTA.—The curves of this diatom prevent its coming well into focus with high powers. The beading of the so-called costæ is not difficult to show; but the relation of the "costæ" to the median band is not clear.

P. VIRIDIS shows fine ribs composed of rows of dots; depressions between each pair of beaded ribs, with beaded rows in depressions. The beaded ribs spring from the median band in various planes, and give its edges a serrated aspect. When a pair of the hair-pin shaped ribs are reddish, the intermediate bead rows are bluish. The median band composed of beads in various planes.

P. NOBILIS.—Very difficult, except in lucky parts, and valves. Much like *viridis*. The beadings curve down to a furrow in the median band. Median band beaded in complicated pattern. The beads only visible when the light comes exactly at the right angle. The rows then seem to go down in a curve, and up again towards the central keel ridge. It is to be expected that the perplexing perspective presented by this object will occasion considerable difference of opinion as to its real shape, but those who have sufficiently good glasses, and take the requisite pains with the illumination, will see that the so-called costæ are very different from any published drawings, and that there are beads in the sup-

posed clear spaces of the median band. Different valves vary considerably in minute detail.

P. MAJOR.—The curves present less difficulties than those of *nobilis*. The beaded ribs very delicate; they commence in curves along margin of median band, something like hooked sticks. Rows of beads in median band.

P. MESOLEPTA.—The best view of this elegant diatom resolves the whole surface into rows of beads, median band included. In form this diatom approximately resembles two skittles from a child's toy box, joined at their bases. The rows of beads run obliquely from median band. Crossing the median band in the centre is another broad band, like two isosceles triangles with truncated apices joined at their narrow ends. The spaces in these bands look clear spaces with insufficient power or imperfect illumination. The lines in the triangular cross-spaces run parallel to the long diameter of the valve.

P. PEREGRINA.—Median band and costæ resolvable into rows of beads.

P. RADIOSA.—Side rows of beads fall in oblique curves from median band that rises like a keel. Median band exhibits rows of fine beads.

P. GIBBA.—Curves different, but structure like *nobilis*, &c.

P. LATA.—This plump little diatom makes the relation of the so-called costæ to the median band very clear. What appear broad costæ under low powers, are found to be loop-like bands of beads, with several rows of beads in the furrows or depressions. Median band consists of rows of fine beads at right angles to costæ. Very troublesome to show the median dots well.

P. FŒDERATA.—All the surface beaded; beads in central nodule arranged in curves. Central nodule difficult.

P. DIVERGENS.—Median band peculiar. As seen with moderate power it looks like a broad, clear space, with two narrow longitudinal furrows, extending one from each end, and not meeting in the middle. Each furrow expands a little at the ends nearest centre. Crossing the centre are two narrow bands. With D eye-piece the $\frac{1}{8}$th resolves the median band into rows of beads which curve round the expanded ends of the two furrows. The so-called costæ resemble those of *nobilis*, but are less out of the horizontal plane.

P. OBLONGA shows relation of so-called costæ to median band very clearly. Symptoms of resolution in median band.

In the preceding cases the structures described are seen with various degrees of distinctness, and some under conditions more or less favourable to optical illusion. It is only by comparing the most distinct and clear exhibitions of the beaded structure with others that are less definite that any prudent microscopist would place much reliance upon the latter.

No tolerably good observer with recent appliances of immersion lenses and condensers can fail to see the composite character of the so-called costæ, though there will be considerable variety of opinion as to the most probable interpretation of the various aspects. Tracing the beading on the median bands is, on the whole, much more troublesome than exhibiting the composite character of the costæ, and it is probably on this point that most doubt will arise. If, however, anyone with good tools and a suitable stock of patience will go through a series of species, it will be readily admitted that in some this medial beading cannot be denied, and others may succeed in seeing it better than the writer has done in the most difficult cases.

The preceding observations have been made with a remarkably fine immersion $\frac{1}{8}$th by Powell and Lealand on their new system. The condenser employed is a $\frac{4}{10}$ths one of Ross, and the usual stop, one radial slot, aperture 109°. The most serviceable eye-pieces were C and D of Ross' scale.

Should these observations meet the eye of Colonel Dr. Woodward, it may induce him to examine some of these diatoms, and give microscopists the benefit of his very remarkable photographic skill. It would be impossible to make drawings that would be accepted as satisfactory, until several good observers of this troublesome class of object have compared notes and decided which, out of many appearances, that look as if they corresponded with fact, may be most prudently trusted.

IV.—*Observations and Experiments with the Microscope, on the Chemical Effects of Chloral Hydrate, Chloroform, Prussic Acid, and other Agents, on the Blood.* By Thomas Shearman Ralph, M.R.C.S., Eng.

On a former occasion, now five years ago, I had the honour of reading before the Medical Society of Victoria a paper entitled "Observations and Experiments with the Microscope, on the effects of Prussic Acid on the Animal Economy," in which I pointed out the specific or chemical action of that agent on the blood, *viz.* that the iron was laid hold of by the cyanogen, and the result was the formation of prussian blue, or some cyanic compound of iron. Accompanying this remarkable change was another, which I also pointed out, that certain oval bodies, closely resembling starch grains, were formed. These bodies turning blue under the action of iodine, and polarizing, were seen to form in the field of the microscope.

After my communication on the effects of prussic acid, I investigated the action of another chemical agent, which exhibits decided effects on the corpuscles of the blood when applied to them out of the body; namely, ammonio-sulphate of copper. When blood is allowed to flow into a solution of this compound, it is found that the contained matter of the red corpuscles cannot pass out; for when blood is drawn and placed in a thin film on glass, and examined under the microscope, it is found that the major part of the corpuscles gives up the contained matter, and the empty cell walls or coverings remain behind. This is well seen on applying a solution of magenta to blood under the microscope; the field becomes occupied by a vast amount of granular matter, coloured red by the dye; while the cell walls or envelopes lie in abundance uncoloured, or at the most presenting to the eye of the observer the red molecule first pointed out by Dr. William Roberts, of Manchester, in 1863, and also brought further into notice by Professor Halford, in 1864, before the Australian Society.* My experiments with this agent, ammonio-sulphate of copper, go to show that while the corpuscles are so acted on that they cannot pass out their contents, yet when magenta is applied, this dye can pass in and colour them; and this coloration shows by its tint the degree of emptiness or fulness of each corpuscle, proving that at the moment when the cupreous solution was added to the fresh-flowing blood, the corpuscles were in different conditions, some perfectly full, while others were partially empty. By means of water the cupreous compound can be washed away, and then these same corpuscles are able to part with their contents, as they do under ordinary circumstances.†

* 'Australian Medical Journal,' vol. ix., 1864. † Ib., vol. xi., 1866.

Subsequently I offered some observations on the action of snake poison on the blood, *i. e.* that it could be compared to that brought about by prussic acid; that this agent, while it attacks the iron in the blood, yet sets up a further action—that of causing the newly-formed red corpuscles to retrograde, as it were, to the condition of the white.*

Here are three important chemical agents, which have been applied to the blood in order to elicit information regarding either its structure or its chemical character, namely:—

Magenta; which was first taken in hand, and which attacks the nuclei of the white corpuscles and colours them; also the granular matter exuded from the red.

Ammonio-Sulphate of Copper; which prevents the egress of the solid portion of the red corpuscles; while at the same time magenta can pass in and colour them effectually.

Prussic Acid; which lays hold of the iron in the blood, withdrawing it from some organic state of combination, giving rise also to the formation of corpora amylacea, or starchy bodies, by some further change effected on the blood constituents.

I feel it is necessary thus to enter upon a *résumé* of what has been done, in order that what follows may be rendered more clear, and that the minds of those to whom I address myself may be satisfied that all the following observations and experiments have proceeded *gradatim*, and owe their origin, and are connected, with my former labours in this field of observation.

In bringing forward the present communication, I feel more and more satisfied of the importance of that mode of investigation which I have employed, that it is one which opens another avenue to the study of physiology, as well as leading us to the ultimate or chemical action of agents on the animal economy.

Experience and observation, based on the separate and combined action of the above-mentioned agents, have satisfied me that some reliable chemical effects may be traced out regarding other agents, whose action on this portion of the animal economy is as yet unknown.

The difficulty hitherto has been to find an agent the effects of which either exceed or distinctly differ from those of any substances hitherto recognized; while, at the same time, the nature and probable action of the new agent should be such as we can trace out without encountering serious difficulty as to its interpretation.

I now proceed to the demonstration of some chemical changes in the blood produced by means of different agents, the effects of which have been hitherto entirely unknown, and which will prove suggestive to the mind of the medical practitioner as soon as he shall have presented to him a further series of experiments carried

* 'Australian Medical Journal,' vol. xii., 1867.

out after the mode of investigation which I have endeavoured to follow, namely, the general action of a chemical substance on the blood withdrawn from the body, and traced out by the microscope; and also the investigation of the action of the same agent on the blood after it has circulated through the animal economy, having been thereby subjected to the continuous action of air during its passage through the lungs. In the former instance we obtain a general kind of action on the blood; in the latter, more positive or distinct effects are presented to our notice, as I shall endeavour to point out by-and-by.

The examination of the blood drawn from the circulation and subjected to the action of a chemical agent does not suffice to show us all that may be produced on it by that agent, and we need, if it is possible, to ascertain and compare its effects after it has circulated in the system. This is true, however, only to a certain extent, for we know that magenta has a remarkable effect on the blood when added to it; but we find no trace of its effects on that fluid when we inject it under the skin, or pass it into the stomach with a view to its absorption and subsequent action on the blood, as the experiments of Professor Halford go to show.

I now pass on to the examination I have made of the action of one important chemical agent, which has only lately been brought before the notice of the medical profession, both here and at home, or rather in Europe. I mean chloral, or as its more chemical name is supposed to be, "Trichloric Aldehyde." This substance is now in use as a hydrate, and its action has been stated to be somewhat like chloroform. When an alkaline solution is added to it, chloroform is set free; hence its proposer, Liebreich, suggested its use: that meeting with alkaline elements in the blood, it might become decomposed into chloroform. This theory, which is a very taking one, was no doubt the cause of the experimental use of this substance. The general experience, however, of the profession is against the idea that it acts as an anæsthetic, but only as a true hypnotic.

I feel inclined to the opinion that though it appears almost certain that chloroform is eliminated in the blood by its decomposition, yet that the action of that agent is considerably modified by the attendant chemical change which necessarily accompanies the decomposition of this agent in the blood. If it is correct that the hydrate of chloral is decomposed by the alkaline state of the blood, then it follows as certainly that the resulting compounds must be chloroform and a formate of some alkali. And if we regard the presence of alkalinity to be normal in the blood, then we obtain not only the chemical or physiological action of chloroform, but we have also to consider what may be the physiological effect of the formate of an alkali, whether of ammonia or potassa, and on such

grounds we may fairly deduce that the physiological effects of chloral on the animal economy must be somewhat different to that of chloroform by itself; hence, perhaps, hypnotism, in place of anæsthesia.*

At the risk of being tedious, I now approach the demonstration I have proposed to make, by reference to the line of my own experience in this matter; and I extract the following from my microscopical note-book:—"August 22nd, 1870.—Hydrate of chloral is a remarkable chemical substance, producing a singular effect on the blood when applied directly to it. A small portion placed on a glass slide and slightly moistened, and then fluid blood added; about one-third of the corpuscles appear to corrugate their solid contents, which then take colour from magenta."

This was the first fact which attracted my notice: the red corpuscles of the blood, when acted upon by magenta, under ordinary circumstances pass out or give up their contents, which then become coloured by the dye. In this instance the dye penetrated the corpuscles, and coloured the material within them.

This effect was sufficient to indicate to my mind the remarkable chemical action of hydrate of chloral; and having made a note of it, I waited until this agent was within my reach for further experiment; for when I made this observation I could only obtain a few grains of it for experimental purposes.

In the following October I had occasion to administer it in small doses, with the view of relieving pain. This enabled me to examine the condition of the blood. The blood, drawn two or three hours after its exhibition, presented a remarkable appearance. In several parts of the field of the microscope, besides *garnet-coloured amorphous particles*, a number of *red-coloured globules* (double the diameter of white corpuscles, and many smaller) were seen; some of these were dark red. This was experiment the first.

Experiment 2.—Hydrate of chloral was exhibited by the stomach to a rabbit; within an hour red masses were seen in the blood, also the presence of starchy bodies was noticed.

Experiment 3.—A frog was subcutaneously injected with hydrate of chloral, with the same results.

* The decomposition of chloral hydrate by ammonia is curious to witness when carried out in the following manner:—A solution of the hydrate should be placed in a narrow tube, about seven or eight inches long, and ammonia added, and the mixture shaken and slightly warmed, when a white cloudiness will make its appearance, and bubbles of gas rise to the surface. If now a little superstratum of water is added, and not allowed to mix with the contents of the tube, the bubbles of gas will be seen passing through this stratum of water, and with a pocket lens the decomposition will be well seen. So soon as a bubble reaches the surface and disappears, from that point there descends an oily-looking fluid (chloroform); but before this reaches the cloudy portion, an amorphous or semi-crystalline material is formed—formate of ammonia; what the gaseous portion is I have not ascertained.

Experiment 4.—Frog immersed in a four-grain solution of the hydrate for some hours, when it was found hypnotic. Blood, nuclei of corpuscles appeared greenish, red particles also seen.

Experiment 5.—Frog killed by hydrate of chloral, after some hours of sleep. Blood from heart decidedly tinged redder than usual; some corpuscles presented reddish dots on their surface; red-coloured masses were noticed all through the blood, as seen before.

Experiment 6.—On self. Three grains of hydrate of chloral were taken about two hours after a meal; the blood was examined every quarter of an hour; at the end of an hour it exhibited decided reaction; blue as well as red particles were seen. When the blood had dried on the glass slide and under the covering glass (which was about two hours after), some spaces, where coagulation had taken place, were filled with fluid presenting either a bluish or reddish tint. The urine also exhibited some dark-coloured and reddish particles.

Experiments 7 *and* 8.—Two rats were killed, one by chloroform, the other by hydrate of chloral, injected subcutaneously. This last took a grain and a half before deep sleep was induced. Blood exhibited ruby-red particles; a few bluish; also starchy bodies in abundance. The urine also showed the same.

The chloroformed rat.—Urine with abundance of starchy bodies, and some blue-coloured particles; blood from lungs—plasma reddish; few starchy bodies; some blue particles; *scarcely* any reddish.

Experiment 9.—Rat injected with hydrate of chloral. Deep hypnotism; blood gave the same results; starchy bodies and red-coloured masses. Ammonia inhaled appeared to increase the production of the red matter. A solution of ammonia injected under the skin appeared to give rise to bright red smears, or fluidity, between the corpuscles. The blood, under the action of ammonia, in both forms of exhibition, seemed to have assumed a redder tint than usual.

Experiment 10.—A newly-born rat was placed in a solution of hydrate of chloral. After some hours the blood exhibited redness in the liquor sanguinis; also some fine red particles and red patches.

Experiment 11.—Hydrate of chloral was evaporated from a slide on to fresh blood held over it; bright red-coloured particles were formed in it.

Experiment 12.—Blood exposed to the vapour of chloroform gave some evidence of red-coloured matter. It appeared to me, at this point of my experiments, that the chemical action of hydrate of chloral on the blood was mainly due to formyl, or formic acid, produced by its decomposition. When more ammonia was introduced into the experiment, a larger production of the red material resulted.

Experiment 13.—Formic acid (obtained from ants) added to blood gave rise to the formation of dark red globules and particles.

Experiment 14.—Lactic acid added to blood yielded red particles; these appeared to increase on the addition of prussic acid; the fluid or plasma appeared redder.

Experiment 15.—Blood, with prussic acid added and then oxalic acid, yielded red-coloured particles.

Experiment 16.—Blood exposed to vapour of hydrate of chloral gave red particles as before; these lost their colour on addition of oxalic acid.

Experiment 17.—Prussic acid and ammonia were mixed together on a slide, and fresh blood added; red particles made their appearance; no starchy bodies; blood corpuscles and plasma redder than usual.

Experiment 18.—Blood, exposed to ammonia vapour, became slightly reddened; prussic acid added in fluid form; blood became decidedly redder; red particles and red plasma resulted.

As far as these experiments had gone, I considered it reasonable to conclude that the decomposition of hydrate of chloral in the blood gave rise to the liberation of formyl, or else formate of ammonia. But what becomes of it? Is it likely to remain in a free and uncombined state? or rather does it not combine with that important element in the blood—iron, producing a formate of iron; or perhaps ammonio-formate of iron?

These decompositions in a highly complex material, as blood, are most difficult of explanation; and it is here I feel we must advance with caution.

The next point to which I directed my attention was to ascertain the action of hydrate of chloral upon a salt of iron; and the following experiments appear to me to support the view I have just expressed.

Experiment.—Chloral dissolved in a little water with ammonia added, was followed by the decomposition of the former; a crystal of sulphate of iron was added, and the effect watched under the microscope; red-coloured particles and amorphous masses of different depths of tint, closely resembling those seen in the blood in the forementioned experiments, made their appearance.

A solution of ammonio-citrate of iron also gives similar results, and somewhat similar also is the action of prussic acid and ammonia conjointly acting on a salt of iron.

Again, being aware from experiments performed in times past, that the presence of iron in vegetable tissues was demonstrable by means of prussic acid and prussic salts, I proceeded to make the following experiment, which, if it does not convince by its single testimony, yet is to my mind highly satisfactory; it is also one of the most remarkable of the kind which I can adduce, in relation to

chemical action on vegetable tissues as revealed by the microscope; when once witnessed it can never be forgotten.

Experiment.—A section of a tender vine was made and placed on a glass slide with water, and chloral hydrate added; a reddish tint pervaded some of the cells; but when ammonia and chloral were added together, the tissue became tinged with a dirty red.

Again: prussic acid and ammonia combined were added to another vine section, and produced a most beautiful and striking reaction. The woody ducts were seen filling up with bright red fluid. In both these instances I have no doubt formyl, or formic acid, attacks the iron combined with the fluids or tissue of the plant.

I can adduce a number of similar experiments, carried out with the same chemical agents, which more or less yield evidence of a similar kind of reaction; but this would prove tedious and superfluous.

I now proceed to sum up my experiments. Hydrate of chloral administered by the stomach or subcutaneously injected, gives rise to the production of bright red or dark red particles, masses, or globules in the blood. Starchy bodies are also met with accompanying these charges. The urine also exhibits these bodies. The same results follow when vapour of hydrate of chloral is applied to fresh-drawn blood.

Ammonia, administered by the lungs, or subcutaneously injected during the action of hydrate of chloral on the animal economy, appears to heighten these effects.

Formic acid added to fresh blood also causes the production of dark red globules and particles.

Lactic acid conjoined with prussic acid does the same.

Prussic acid and ammonia conjoined yields the same results.

The action of hydrate of chloral, while decomposing under ammonia on a salt of iron, presents changes which to my mind are identical.

The chemical effects of hydrate of chloral and ammonia, of prussic acid and ammonia, on some vegetable tissues, appear to be much the same in character as those produced in the blood, minus, of course, the solid albuminous matter. All these results I refer to the action of formyl or ammonio-formate on the iron in the blood, or in the vegetable tissues.

The decomposition of lactic acid with prussic acid can supply chemically the elements necessary for the production of formyl or formate of ammonia; as also prussic acid and ammonia.

There are one or two more experiments to which I must refer, *i.e.* the chloroformed rat, in which the blood was noticed to be reddish, but scarcely any red matter was seen. It would appear the chemical condition of the blood is not capable of readily decomposing chloroform; such also is the case, I believe, with hydrate of

chloral applied to the blood out of the body; but when the vapour of chloroform, or the hydrate of chloral is applied, then the red particles make their appearance.

Here is another remarkable occurrence which receives its solution from the forementioned experiments. Some blood was accidentally examined after wine (Reisling) had been taken; this was with the view of exhibiting the action of prussic acid on iron in the blood; but it was noticed little or no reaction could be found after its application; but a good many red-coloured globules and particles were seen, just as if chloral had been taken. In consequence of this, a small drop of the wine was added to a little blood fresh drawn; the changes seen under the microscope were most remarkable. Abundance of globules of a dark-red or brown colour made their appearance, as also red amorphous masses or particles. Gas also was given off in the neighbourhood of the globules. Some of these bubbles contained a bluish fluid; the nuclei of the white corpuscles were bluish.

The experience I have already gained in carrying out these experiments leads me to see that the condition of the blood recognizably varies from day to day, from the effects of food, &c.; for the varying degrees of success which have attended a number of experiments performed with the same chemical agent, on the same individual, point to the great probability of the variable condition of the blood, when that individual has been the subject of variety in diet, or degrees of fatigue of mind or body.

Another consideration which presents itself to my mind is, that just as we now test the condition of the urine in order to ascertain what is being eliminated from the body of a patient, so will the physician find it useful occasionally to test, by means of reagents, the condition of the blood of his patient, in order to verify the character of some obscure symptoms. Even at this period of my experience, I have reason to believe it is possible by means of agents previously administered, to prolong the hypnotic action of chloral, or to prevent or modify it in a great degree.

Thus, I believe, I have at least been able to give demonstration to the theory of Liebreich, who, by his chemical knowledge, has enabled the medical practitioner to employ a remarkable agent, one which promises to be a sister companion to chloroform in alleviating the ills to which flesh is heir. I hope I may be fortunate enough to arouse the attention of my professional brethren to the investigation of the chemical action of remedies on the blood, and thereby, perhaps, lead on to a more rational and satisfactory mode of treating some diseases, which in time to come, I believe, will be attacked directly through the blood itself.—*Read before the Medical Society of Victoria.*

V.—*Floscularia Cyclops: a New Species.*

By Charles Cubitt, F.R.M.S.

(*Taken as read before the* Royal Microscopical Society.)

Plate XCIII.

I have lately found associated with *F. coronetta* another and a larger floscule than that elegant species, and it is impossible to apply the specific title of any known floscule to this particular form, for while the disk resembles somewhat that of *F. ornata*, this new species differs essentially from that in many important details, which will be clearly seen and appreciated on comparing it with *F. ornata*, by referring to Plate XCIII., Figs. 1 and 2, which figures have been drawn to the same scale and placed in juxtaposition for the purpose of conveying a clear perception of their respective forms and proportions; and in preparing these I have assumed the altitude of *F. ornata* to be $\frac{1}{30}$th, a length they frequently though not constantly attain, due no doubt to different localities and habits, for in some places they never exceed $\frac{1}{60}$th, or just half the size shown by Fig. 2.

The first point which must strike the observer is the extraordinary height of the animal, and while the size of the whole form greatly exceeds that of *F. ornata*, there is a striking resemblance between the disks of these two species; but while the disk of *F. Cyclops* is proportionately larger, the so-called *dorsal* lobe is considerably smaller than that of *F. ornata*, and the investing case is increased in a still greater degree than the occupant itself. The case is very distinct, and is seen to be palpably invaginated by every retraction of the occupant, especially after acts of evacuation, when portions of the voided matter remain clinging to the sides of the case within the invaginated portion, and are not dispersed throughout the hyaline medium which fills the integument.

I do not consider it necessary to enter into a more detailed account of this particular form, which is essentially of the same organization as the other members, but I desire to claim the privilege of identifying it in this record by a specific title, and, in consequence of its great bulk, have selected that of *Cyclops;* and however this may be objected to, on a first consideration, in the generally accepted notion of the possession of a single eye by the Gods in question (for there were three of them), this traditionary assumption of a single eye is erroneous; these three brothers adopted the custom of wearing small bucklers of steel wherewith to cover their faces, and these bucklers had one small opening in the centre, and only so far corresponded with a single eye; and like the brothers Arges, Brontes, and Steropes, our valiant *Cyclops* has two eyes.

VI.—*Mr. Tolles' "Experiments on Angular Aperture."*

By F. H. Wenham, Vice-President R.M.S.

The above essay would not have required any special notice from me, had not reference been made to my name. I consider that I have already said enough to settle the question in minds familiar with optical science, but there are others who might suppose, from the evidence of these experiments, that I have assumed things incorrectly, for there appears to be a lack of discrimination in this subject that quite justifies the remark made by the Rev. S. L. Brakey "as showing the incredibly low level at which the *scientific* knowledge of optics exists amongst microscopists."

Mr. Tolles' communication is set forth in an unassuming way, that does not provoke any hard strictures, but it enters within the lines of a controversy that has been long and hotly contested in reference to a subject which it would seem remains yet to be understood.

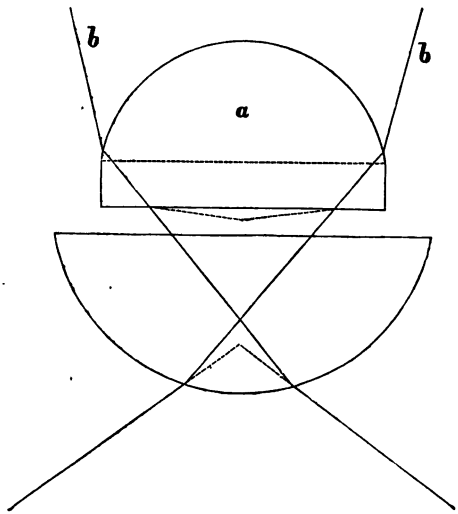
As shown by the figure, Mr. Tolles fixes before the front lens of an objective, a plano-convex lens nearly hemispherical, and then measures the aperture. "On testing the angle, only 80° (or at most, less than 82°) was obtainable. Were the plano-convex removed, the angle indicated would be 170° upwards. This was verified carefully." There is a simple honesty and truth in this experiment that is very pleasing. Half of 82°, or 41°, would be about the angle of total reflexion from the flat surface of the plano-lens, at which a veil of utter darkness would be thrown between the object-glass and the light, and none could pass beyond, so that after this the plano-convex had nothing whatever to do with apertures exceeding 82°.

We next come to the immersion question. I must compliment Mr. Tolles on the accuracy of his little diagram. The hemispherical lens is, I have no doubt, correct, and so is the front of the objective, both for diameter and thickness, as it should be, coming from a professional maker. Mr. Tolles says, "When the air in this interspace (*i. e.* between the lenses) is replaced by water, the angle becomes 100°, or a little more (an after-note states it as 110). . . . It seems incontestable, at all events, that more than 82° of angular pencil *can* traverse the balsam-mounted object, and be transmitted by the immersion objective to the eye of the observer."

Though I saw at a glance why this assertion must be incorrect, and could explain the reason in a few words, yet as Mr. Tolles has given me facilities for doing so, I elucidate the point by the aid of a diagram, exactly copied, four times the size of his own figure. *a*, Fig. 1, is the front lens of the object-glass; *b*, *b*, rays; these, after being refracted by the convex and flat surfaces, would con-

verge in the dry lens at an angle of 170°, as shown by the dotted lines. Now let us introduce water between the lenses. Instantly a change takes place; no one, I think, will have the hardihood to maintain that the rays will hold their former position: on the contrary, they will converge at an angle of near 80°, and meet at a point in the body of the under lens. They cross and reach the lower hemispherical surface;* here they are refracted more outwards, and form an angle with each other of 108°, only two degrees short of the 110° given by the experiment. This angle is obtained by careful projection of the rays, and is the one mistaken by Mr. Tolles for the representative of the extreme rays of the immersion objective. The microscope body is presumed to have been rotated on a sector as usual, but in order to make the thing more plain to those who do not perceive such matters very clearly, we will suppose the light itself to be traversed instead (which is very frequently done); it must be evident that this will have to be moved a *greater distance*, on account of the bending outwards of the rays by the lower surface of the hemispherical lens, and so give a false indication of increased aperture.

FIG 1.

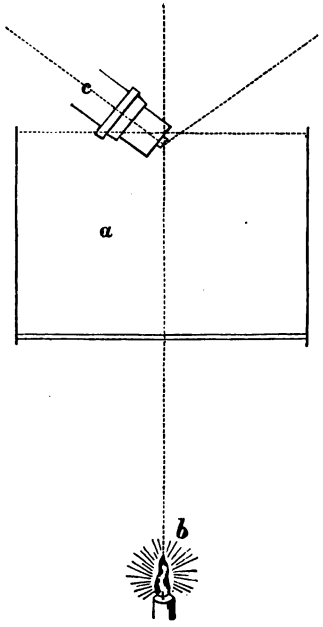


The loss of aperture on balsam-mounted objects was demonstrated by me on correct optical laws known ages ago, and I am astonished that in the nineteenth century anyone can dare to dispute it as a fact; but as it seems necessary to give an experiment to convince the sceptical that when the refraction of the front surface of an object-glass is partly neutralized by water contact, or destroyed by balsam, and the angle of aperture most woefully reduced on objects immersed therein, I have done this as follows:—

* A slight displacement of the ray, too small to be shown in the diagram, will be caused by the water-film; but this does not alter the angular direction *in* the hemispherical lens, though it may slightly increase the angle on finally leaving it.

a, Fig. 2, is a tank with a plate-glass bottom filled with water,* *b* is the light some distance below on the floor; the sector for measuring the apertures is set vertically, so that the nose of the object-glass *c*, may remain immersed in the water during the rotation. The following are some results:—A $\frac{1}{12}$th of 170° (immersion front) showed an apparent aperture of 100°; a $\frac{1}{8}$th of 130° = 87°, and a $\frac{4}{10}$th of 90° = 67°. If a higher refractive medium than water had been employed, the angles would have been still less, and with Canada balsam, or something of the *same* refractive power of the glass, the aperture from 170° could not exceed 80°. This experiment would have been more inconvenient to try, and I did not think it worth while to run the risk of injuring my object-glasses for demonstrating a simple fact in known optical laws quite incontrovertible. About 80° is the utmost aperture that we can expect to obtain for an object *mounted in balsam*; and the principle does not differ, whether we employ an immersed front or not. The latter, of course, has the advantage in transmitting more light, and allows greater control over the aberrations. A tank might be used with a glass side, and the sector kept horizontal as usual, but this requiring an elastic water-tight connection from the object-glass, would be troublesome.

FIG. 2



Messrs. Tolles and Stodder have girded on a convex front, and then ventured forth to make a stand against my unwelcome statements—that the aperture of object-glasses *is* reduced on balsam-mounted objects, and that there is *no* subsequent increase of this aperture by using an immersion lens. I had not the pleasure of shaking hands with them before the collision, but in the absence of this ceremony, I hope they may take the reception that they have met with in good part.

* The glass-bottomed metal pot, used by fastidious beer-drinkers, will answer the purpose, but I do not know whether our Transatlantic friends are familiar with this.

VII.—*On the Microscopical Structure and Composition of a Phonolite from the "Wolf Rock."* By S. Allport, F.G.S. With a Chemical Analysis by Mr. J. A. Phillips.

The rock described in the following paper is, I believe, new to British petrology; and as the value of microscopical analysis is not yet fully recognized, a detailed description may be acceptable to many readers. The specimens examined were kindly sent to me by Mr. J. A. Phillips, together with a chemical analysis, which, with his permission, is also added.

The "Wolf" is a rugged rock lying about nine miles south-east of the Land's End, and covered by the sea at high water. At low water of spring tides its length is about 175 feet, and its breadth 150 feet. Its highest point at low water is 17 feet above the level of the sea, whilst at high water it is covered by it to a depth of 2 feet.

Examined by the eye or simple lens, the rock is seen to consist of a yellowish-grey compact base, in which crystals of clear glassy felspar are imbedded; they exhibit no striæ; their fracture is sharp and splintery.

A thin section examined in polarized light with crossed prisms exhibits a beautiful group of crystals of felspar and nepheline porphyritically imbedded in a fine-grained matrix composed of minute crystals of nepheline, felspar, and hornblende; when cut very thin, the hornblende alone exhibits colours, the hexagonal sections of nepheline being black, the rectangular white; the felspar is also either dark or light, and the general appearance is that of a mosaic of dark and light stones interspersed with small brilliant-coloured crystals of hornblende; the whole forming a matrix in which the larger crystals are set. In thicker sections the felspar and nepheline display fine colours, but the minute structure is not so well seen.

The microscopic constituents are for the most part evenly distributed throughout the base, but not unfrequently they are crowded together along the sides of the larger crystals and irregular grains of nepheline. This is an important fact, as it clearly indicates that both the nepheline and the smaller crystals had been formed while the surrounding mass was still in a plastic state; it would also appear that the felspar was the last to crystallize, as it frequently encloses crystals of nepheline and hornblende, which must have been caught up in it at the time of consolidation.

The nepheline occurs in the sections in hexagonal and rectangular forms, or as imperfect crystals and irregular grains; some are perfectly clear and transparent, but the greater number appear to be filled with a fine grey dust; it is sometimes equally distributed, but

is also frequently collected together so as to form a dark, or even black mass in the centre, the edges of which are sharply defined, and correspond exactly with those of the crystal. Hexagonal crystals, for example, exhibit a border filled with fine grey dust, and a central portion occupied by a well-defined black hexagon; or there is sometimes a black band running parallel with and at some distance from the sides, the central and outer portions of the crystal being occupied by the grey dust. With a magnifying power of 800, a portion of the dust is resolved into minute granules, having a translucent centre surrounded by a dark ring; they are therefore probably *glass cavities*. It is especially worthy of remark that this grey dust occurs in precisely the same way in the nepheline of the basalts and phonolites of Tertiary age, and from widely separated localities. The clear crystals frequently exhibit faint lines parallel with the sides, and they often enclose slender acicular prisms; compound and twin crystals are not uncommon.

The felspar is perfectly clear and transparent, and evidently belongs to the orthoclase group; no striæ are observable, and one or two crystals give an angle of 90° by measurement with the goniometer. The prisms are frequently much fractured transversely to the long axis, and in this respect, and in their optical character, they closely resemble the sanidine of some trachytes and phonolites. Twin crystals showing different colours on opposite sides of the plane of junction are not uncommon. The felspar contains numerous glass cavities and extremely minute crystals, which are sometimes irregularly distributed, but are also frequently arranged in rows parallel with the edges of the larger crystals; the latter have also caught up crystals of nepheline, and a few long slender prisms, probably apatite.

The hornblende occurs in minute green prisms, varying in length from $\frac{1}{600}$ to $\frac{1}{200}$th of an inch, and in width from $\frac{1}{3000}$ to $\frac{1}{1000}$ in. They are regularly interspersed with the other constituents of the base, but occasionally great numbers of them are crowded together in closely compacted groups, having a nucleus of black grains of magnetite; a few prisms may also be seen imbedded in the nepheline and felspar.

There may perhaps be some little uncertainty whether this pyroxenic mineral be hornblende or augite. I have not met with any reliable angles, as most of these minute prisms appear to be broken or imperfectly formed at the ends; a few of larger size are, however, distinctly dichroic, and would therefore appear to be hornblende.

The greater part of the mass of the rock is seen to consist of nepheline; the crystals forming the base vary in size from the $\frac{1}{150}$ to $\frac{1}{300}$ in. across, but there are perfect hexagons which do not measure more than the $\frac{1}{2000}$th of an inch; most of them are

indistinct in outline when seen by ordinary light, but become well defined when examined with a half-inch objective between crossed prisms.

Two analyses of this rock afforded Mr. J. A. Phillips the following results:—

Sp. Gr. = 2·54.

	I.	II.
*Water	2·05 per cent.	2·05
Silica	56·46 „	56·40
Alumina	22·29 „	22·20
Peroxide of iron	2·70 „	2·61
Protoxide of iron	·97 „	·97
Manganese	Trace	Trace
Lime	1·47 „	1·35
Magnesia	Trace	Trace
Phosphoric acid	Trace	Trace
Potassa	2·81 „	2·73
Soda	11·13 „	11·11
	99·88	99·42

Anyone who has made a careful examination of the Tertiary phonolites, or is acquainted with Professor Zirkel's researches on them, will at once recognize the identity of their mineralogical composition with the rock here described, and will be struck with the thoroughly characteristic appearance of the nepheline, which is absolutely the same in both. In fact, no one would hesitate to call it a phonolite, if it were known to be of Tertiary age. The age, however, is unknown, and likely to remain so, for the rock stands alone in the sea, and its actual relations with others cannot be observed. Situated between the Land's End and Scilly Islands, it is in a Palæozoic district, disturbed and penetrated in all directions by granites, porphyrites, and diorites; few, therefore, will hesitate to place it among the older series of igneous rocks. It is at present the practice among many petrologists to name rocks according to their supposed geological age; a dark-coloured augitic rock, for example, would be a *basalt* if of Tertiary age, but must be a *melaphyr* or *aphanite*, if of some indefinite early age. In accordance with this absurd system, the rock in question would probably be called a *Foyaite*, if it were known to be old, as it agrees well with descriptions of that rock, except that the elæolite is here represented by nepheline crystals which cannot be distinguished from those of true phonolites.

After some hesitation, I have adopted the name of *porphyritic phonolite* for this rock, and will take the present opportunity of suggesting that one name only should be assigned to all igneous rocks composed of the same constituent minerals, irrespectively of their age; or, in other words, that we should assimilate the nomenclature

* Of which ·94 was lost in water-bath.

to that employed in the sedimentary rocks. We speak of Carboniferous or Tertiary sandstones, &c., why not Carboniferous or Tertiary dolerites or melaphyres? When the age cannot be precisely ascertained, an approximation may generally be made, and such terms as *post-Carboniferous*, *ante-Triassic*, &c., might be used.

If some such system were adopted, all the basic, augitic rocks containing much iron oxide, would form one group, and we should get rid of a number of useless names which have been applied to rocks in utter ignorance of their mineralogical composition or structure.

Until quite recently such a suggestion could not have been adopted, as there were no means of ascertaining with certainty the constituents of the fine-grained rocks; but now that improved methods of microscopical research are available, it is quite time that the unscientific nomenclature still in use should be supplanted by one more in accordance with the present state of knowledge.—*Geological Magazine*, June.

VIII.—*On Spore-cases in Coals.*

By J. W. Dawson, LL.D., F.R.S.

When in London, last year, Prof. Huxley was kind enough to show me some remarkably beautiful slices of coal mounted by his assistant, Mr. Newton, and showing with great distinctness multitudes of spore-cases and spores, some of them very well preserved. He further stated to me his belief that such material had been largely or mainly instrumental in the production of Coal. At the time I declined to accept this conclusion, on the ground that the specimens probably represented layers of coal exceptionally rich in spore-cases; and that even in these specimens a large quantity of matter was present which long experience in the examination of coals enabled me to recognize as cortical or epidermal matter, which I had previously shown by my examination of the coals of Nova Scotia to be the principal ingredient in ordinary coal. I promised, however, on my return to Canada, to look over my series of preparations of coal, with a view to the occurrence of spore-cases, and also to make trial of the somewhat improved method of preparation employed by Mr. Newton. On my return I gave the results of my examination to Prof. Huxley, in a letter which he has quoted in the brilliant exposition of his observations and conclusions in the 'Contemporary Review' for November,* and which will probably give a tone to the representations of popular writers on this subject for

* In the quotation the word "cubical" has been substituted for "cortical."

some time. While, however, admitting the great interest and importance of Prof. Huxley's observations, and prepared to contribute some additional illustrations of the occurrence of spore-cases in coal, I think it well to direct attention anew to the actual composition of the substance, as proved by its mode of occurrence, and illustrated by my own extensive series of observations on the coals of Nova Scotia and Cape Breton, including the series of eighty-one seams exposed at the South Joggins, the whole of which I have examined *in situ* and under the microscope.

The occurrence of bodies supposed to be spore-cases in coal, is, as Prof. Huxley states, no new discovery; but in reality these may be said to be the first organisms recognized by any microscopic observer of coal—that is, if all the clear spots and annular bodies seen in slices of coal are really spore-cases. They were noticed by Morris as early as 1836, and they had been observed and described long before by Fleming in Scotland. Goeppert mentioned and figured them in his 'Treatise on Coal' in 1848. Balfour described them in 1859 as occurring in Scottish coals, and Quekett figured them in his account of the Torbane Hill mineral in the same year. In 1855 the latter microscopist showed me in London slices exhibiting round bodies of this kind, very similar to those now described by Huxley; but at that time I regarded them as concretionary, though Prof. Quekett was disposed to consider them organic. Mr. Carruthers has summed up most of these facts in his account of his genus Flemingites in the 'Geological Magazine' for October, 1865. The subject has also attracted the attention of microscopists in connection with the Tasmanite, or "white coal" of Tasmania, which is composed in great part of the spore-cases of ferns.

I suppose that the oldest spore-cases known are those described by Hooker, from the Ludlow formation of the Upper Silurian; but these, if really spore-cases, are different in structure from those ordinarily found in the coal formation, more especially in the great thickness of their walls, and I am not aware that they have anywhere been found in considerable quantities.

The oldest bed of spore-cases known to me, is that at Kettle Point, Lake Huron. It is a bed of brown bituminous shale, burning with much flame, and under a lens is seen to be studded with flattened disk-like bodies scarcely more than a hundredth of an inch in diameter, which under the microscope are found to be spore-cases, slightly papillate externally, and with a point of attachment on one side and a slit more or less elongated and gaping on the other. I have proposed for these bodies the name *Sporangites Huronensis*. When slices of the rock are made, its substance is seen to be filled with these bodies, which, viewed as transparent objects, appear yellow like amber, and show little structure, except that the walls can, in some cases, be distinguished from the internal cavity, and the

latter may be seen to enclose patches of flocculent or granular matter. In the shale containing them there are also vast numbers of rounded translucent granules which may be the escaped spores.

The bed at Kettle Point is stated in the report of the Geological Survey to be 12 to 14 feet in thickness; but to what degree either in its thickness or horizontal extent it retains the characters above described, I do not know. It belongs to the Upper Devonian, being supposed to be a representative of the Genesee slates of New York. It contains stems of *Calamites inornatus* and of a *Lepidodendron*, obscurely preserved, but apparently of the type of *L. Veltheimianum*, and possibly the same with *L. primævum* of Rogers. The spore-cases are not improbably those of this plant, or of the species *L. Gaspianum*, which belongs to the same horizon, though not found at this locality. The occurrence of this bed is a remarkable evidence of the abundance of Lycopodiaceous trees, whose spores must have drifted in immense quantities in the winds, to form such a bed. It is to be observed, however, that this is not a bed of coal, but a bituminous shale of brown colour, and with pale streak, no doubt accumulated in water, and even marine, since it contains *Spirophyton** and shells of *Lingula*. In this it agrees with the Australian Tasmanite, which, though composed in great part of spore-cases of ferns, is, as I am informed by Mr. Selwyn, an aqueous deposit, containing marine shells.

There is, however, one bed of true coal known in the Devonian of Eastern America, that of Tar Point, Gaspé, and it is curious to observe that this is not composed of spore-cases, but of successive thin layers of rhizomata and stems of *Psilophyton*, with occasional fragments of *Lepidodendron* and *Cyclostigma*. Rounded disks, which may be spore-cases, occur in it, but very rarely. In the bituminous shales associated with this coal the microscope shows amber-coloured flakes of irregular form, but these are easily ascertained to be portions of the epidermis of Psilophyton, or of the chitinous crusts of crustaceans which abound in these beds.

Ascending to the Lower Carboniferous (sub-carboniferous), there are great quantities of rounded spore-cases of the size of mustard seeds (*Sporangites glabra* of my papers) in the rocks of Horton Bluff and Lower Horton, Nova Scotia. They are sometimes globular, and filled with pyrite of a granular texture which perhaps represents the original cellular structure or the microspores. In other cases they are flattened and constitute thin carbonaceous layers. They are almost without doubt the spore-cases of *Lepidodendron corrugatum*, which abounds in the same beds, and constitutes in one place a forest of erect stumps. I described them in a paper "On the Lower Carboniferous of Nova Scotia," in the 'Proceedings of the Geological Society of London' for 1858, though not then aware of

* The well-known *Cauda-galli* fucoid.

their true nature, which was, however, recognized by Dr. Hooker in some specimens which I had sent to London.

In my paper "On the Conditions of Accumulation of Coal,"* I proposed the name *Sporangites* for these bodies, in consequence of the difficulty of referring them certainly to any generic forms. Carruthers had, in Oct. 1865, described a cone containing rounded spore-cases of not dissimilar type, under the name *Flemingites*. In the paper above referred to, I stated that out of eighty-one coals of the South Joggins Section examined by me, I recognized these bodies and other fruits or sporangia in only sixteen; and of these only four had the rounded Lycopodiaceous spore-cases similar to those of Flemingites. These are the following:—

(1.) Coal group 12, of Division IV., has a bed of coal one foot thick, of which some layers are almost wholly composed of *Sporangites papillata*.

(2.) Coal group 13, Div. IV., has in some layers great quantities of *Sporangites glabra*, especially in the shaly part of the coal.

(3.) In Coal group 14, Div. IV., a shaly parting contains great numbers of similar sporangites.

(4.) In Coal group 15*a*, Div. IV., the shaly roof abounds in sporangites, but I did not observe them in the coal itself.

In addition to these cases, all of which curiously enough occur in one part of the section, and among the smaller coals, I have noted the occurrence of clear amber spots in several of the compact coals, but I did not regard these as certainly organic, suspecting them to be rather concretionary or segregative structures.

The great coal beds of Pictou are, in so far as my observation has extended, remarkably free from indications of spore-cases, and consist principally of cortical and ligneous tissues with layers of finely comminuted vegetable matter. A layer of cannel, however, from a bed near New Glasgow has numerous flattened amber-coloured disks, which may be of this character. In those of Cape Breton, the yellow spore-case-like spots are much more abundant; but these coals I have less extensively examined than those of the mainland of Nova Scotia. Of American coals, the richest in spore-cases, that I have seen, is a specimen from Ohio, which contains many large spore-cases, and vast numbers of more minute globular bodies apparently macrospores. It quite equals in this respect some of the English coals referred to by Huxley. I have also a specimen of anthracite from Pennsylvania, full of spore-cases, some of them retaining their round form and filled with granular matter which may represent the spores.

It is not improbable that sporangites or bodies resembling them, may be found in most coals; but the facts above stated indicate that their occurrence is accidental, rather than essential to coal accu-

* 'Proceedings of Geological Society of London,' May, 1866.

mulation, and that they are more likely to have been abundant in shales and cannel coals, deposited in ponds or in shallow waters in the vicinity of Lycopodiaceous forests, than in the swampy or peaty deposits which constitute the ordinary coals. It is to be observed, however, that the conspicuous appearance which these bodies, and also the strips and fragments of epidermal tissue, which resemble them in texture, present in slices of coal, may incline an observer, not having large experience in the examination of coals, to overrate their importance, and this I think has been done by most microscopists, especially those who have confined their attention to slices prepared by the lapidary. One must also bear in mind the danger arising from mistaking concretionary accumulations of bituminous matter for sporangia. In sections of the bituminous shales accompanying the Devonian coal above mentioned, there are many rounded yellow spots, which on examination prove to be the spaces in the epidermis of Psilophyton through which the vessels passing to the leaves were emitted. To these considerations I would add the following, condensed from my paper above referred to, in which the whole question of the origin of coal is fully discussed.*

(1.) The mineral charcoal or "mother coal" is obviously woody tissue and fibres of bark; the structure of the varieties of which and the plants to which it probably belongs, I have discussed in the paper above mentioned.

(2.) The coarser layers of coal show under the microscope a confused mass of fragments of vegetable matter belonging to various descriptions of plants, and including, but not usually largely, sporangites.

(3.) The more brilliant layers of the coal are seen, when separated by thin laminæ of clay, to have on their surfaces the markings of Sigillariæ and other trees, of which they evidently represent flattened specimens, or rather the bark of such specimens. Under the microscope, when their structures are preserved, these layers show cortical tissues more abundantly than any others.

(4.) Some thin layers of coal consist mainly of flattened layers of leaves of *Cordaites* or *Pychnophyllum*.

(5.) The Stigmaria underclays and the stumps of Sigillaria in the coal roofs equally testify to the accumulation of coal by the growth of successive forests, more especially of Sigillariæ. There is on the other hand no necessary connection of sporangite beds with Stigmarian soils. Such beds are more likely to be accumulated in water, and consequently to constitute bituminous shales and cannels.

(6.) Lepidodendron and its allies, to which the spore-cases in question appear to belong, are evidently much less important to coal accumulation than Sigillaria, which cannot be affirmed to have pro-

* See also 'Acadian Geology,' 2nd edit., pp. 138, 461, 493.

duced spore-cases similar to those in question, even though the observation of Goldenberg as to their fruit can be relied on; the accuracy of which, however, I am inclined to doubt.

On the whole, then, while giving due credit to Prof. Huxley and those who have preceded him in this matter, for directing attention to this curious and no doubt important constituent of mineral fuel, and admitting that I may possibly have given too little attention to it, I must maintain that sporangite beds are exceptional among coals, and that cortical and woody matters are the most abundant ingredients in all the ordinary kinds; and to this I cannot think that the coals of England constitute an exception.

It is to be observed, in conclusion, that the spore-cases of plants, in their indestructibility and richly carbonaceous character, only partake of qualities common to most suberous and epidermal matters, as I have explained in the publications already referred to. Such epidermal and cortical substances are extremely rich in carbon and hydrogen, in this resembling bituminous coal. They are also very little liable to decay, and they resist more than other vegetable matters aqueous infiltration; properties which have caused them to remain unchanged and to resist the penetration of mineral substances more than other vegetable tissues. These qualities are well seen in the bark of our American white birch. It is no wonder that materials of this kind should constitute considerable portions of such vegetable accumulations as the beds of coal, and that when present in large proportion they should afford richly bituminous beds. All this agrees with the fact, apparent on examination of the common coal, that the greater number of its purest layers consist of the flattened bark of Sigillariæ and similar trees, just as any single flattened trunk imbedded in shale becomes a layer of pure coal. It also agrees with the fact that other layers of coal, and also the cannels and earthy bitumens appear, under the microscope, to consist of finely comminuted particles, principally of epidermal tissues, not only from the fruits and spore-cases of plants, but also from their leaves and stems. The same considerations impress us, just as much as the abundance of spore-cases, with the immense amount of the vegetable matter which has perished during the accumulation of coal, in comparison with that which has been preserved.

I am indebted to Dr. T. Sterry Hunt for the following very valuable information, which at once places in a clear and precise light the chemical relations of epidermal tissue and spores with coal. Dr. Hunt says:—"The outer bark of the Cork-tree and the cuticle of many if not all other plants consists of a highly carbonaceous matter, to which the name of *suberin* has been given. The spores of Lycopodium also approach to this substance in composition, as will be seen by the following, one of two analyses by Duconi,* along with

* Liebig and Kopp, Jahresbuch, 1847–48.

which I give the theoretical composition of pure cellulose or woody fibre, according to Payen and Mitscherlich, and an analysis of the suberin of Cork, from *Quercus suber*, from which the ash and 2·5 per cent. of cellulose have been deducted.*

	Cellulose.	Cork.	Lycopodium.
Carbon	44·44	65·73	64·80
Hydrogen	6·17	8·33	8·73
Nitrogen	—	1·50	6·18
Oxygen	49·39	24·44	20·29
	100·00	100·00	100·00

"This difference is not less striking when we reduce the above centesimal analyses to correspond with the formula of cellulose, $C_{24}H_{20}O_{20}$, and represent Cork and Lycopodium as containing 24 equivalents of carbon. For comparison I give the composition of specimens of peat, brown coal, lignite, and bituminous coal.†

Cellulose	$C_{24}H_{20}O_{20}$
Cork	$C_{24}H_{18\frac{3}{10}}O_{6\frac{7}{10}}$
Lycopodium	$C_{24}H_{19\frac{4}{10}}NO_{5\frac{6}{10}}$
Peat (Vaux)	$C_{24}H_{14\frac{4}{10}}O_{10}$
Brown coal (Schröther)	$C_{24}H_{14\frac{3}{10}}O_{10\frac{6}{10}}$
Lignite (Vaux)	$C_{24}H_{11\frac{2}{10}}O_{6\frac{4}{10}}$
Bituminous coal (Regnault)	$C_{24}H_{10}O_{3\frac{3}{10}}$

"It will be seen from this comparison that, in ultimate composition, Cork and Lycopodium are nearer to lignite than to woody fibre; and may be converted into coal with far less loss of carbon and hydrogen than the latter. They in fact approach closer in composition to resins and fats than to wood, and moreover like those substances repel water, with which they are not easily moistened, and thus are able to resist those atmospheric influences which effect the decay of woody tissue."

I would add to this only one further consideration. The nitrogen present in the Lycopodium spores no doubt belongs to the protoplasm contained in them, a substance which would soon perish by decay; and subtracting this, the cell-walls of the spores and the walls of the spore-cases would be most suitable material for the production of bituminous coal. But this suitableness they share with the epidermal tissue of the scales of strobiles, and of the stems and leaves of Ferns and Lycopods; and above all, with the thick corky envelope of the stems of Sigillariæ and similar trees, which, as I have elsewhere shown,‡ from its condition in the prostrate and erect trunks contained in the beds associated with coal, must have been highly carbonaceous and extremely enduring and impermeable to water. In short, if instead of "spore-cases" we read "epidermal tissues in

* Gmelin, 'Handbook,' xv., 145. † 'Canadian Naturalist,' vi., 253.

‡ "Vegetable Structures in Coal," 'Journ. Geol. Soc.,' xv., 626; "Conditions of Accumulation of Coal," ib., xxii., 95; 'Acadian Geology,' 197, 464.

general, including spore-cases," all that Huxley has affirmed will be strictly and literally true, and in accordance with the chemical composition, microscopical characters, and mode of occurrence of coal. It will also be in accordance with the following statement, which I may be pardoned for quoting from my paper "On the Structures in Coal," published in 1859:—

"A single trunk of Sigillaria, in an erect forest, presents an epitome of a coal-seam. Its roots represent the Stigmaria underclay; its bark the compact coal; its woody axis, the mineral charcoal; its fallen leaves (and fruits), with remains of herbaceous plants growing in its shade, mixed with a little earthy matter, the layers of coarse coal. The condition of the durable outer bark of erect trees concurs with the chemical theory of coal, in showing the especial suitableness of this kind of tissue for the production of the purer compact coals. It is also probable that the comparative impermeability of the bark to mineral infiltration, is of importance in this respect, enabling this material to remain unaffected by causes which have filled those layers, consisting of herbaceous materials and decayed wood, with pyrites and other mineral substances."—*American Journal of Science*, No. 4, Vol. I.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Retrograde Development of Marine Bryozoa.—In the first number of vol. xxi. of Siebold u. Kölliker's 'Zeitschrift,' Claparède, who with the exception of Nitzsche is the only writer who has studied the Bryozoa since the publication of the capital papers of Smitt, gives us most interesting contributions to their history. While on the main points he completely agrees with the views taken by Smitt of the polymorphism of the species, their mode of budding and general embryonic development, yet in some points not satisfactorily determined by Smitt, such as the relations of the various cells (zoœcia) to one another, the nature of Smitt's "mörka kroppar," dark bodies, and "fett kroppar," he has new observations differing somewhat from those of Smitt. The most interesting facts (which are recorded in 'Silliman's American Journal,' by a writer who signs himself L. L. G., and who is perhaps Agassiz) are those concerning a sort of retrograde development, a resorption of the digestive cavity in the older cells, the gradual disappearance of the lophophore, resulting in cells usually considered as dead but in reality having latent life, and where alone the fatty bodies of Smitt, which play such an important part in the embryology of Bryozoa, are developed. These cells apparently pass through stages identical with those produced by budding at the youngest extremity of the colony, with the difference that in one case the cell is immature, while in the other it is fully developed. The resorption is frequently accompanied by peculiar changes in these cells, and is confined to the older portions of the Bryozoan colony in which the lateral connection between the cells for exchange of fluids between the cells provided with digestive cavities and those cells containing latent life, is very strikingly shown, thus forming a complete circulation between the most distant parts of the colony. He also confirms the nature of the colonial nervous system, first traced by Fritz Müller, and shows its existence among the Chilostomata, where it had only been traced by Smitt before. Claparède closes this interesting paper by giving us the complete development of Bugula, with larger, more accurate, and at the same time more intelligible figures than we have had of the early development of any one species of marine Bryozoa thus far. He has, however, not been able to decide positively the nature of the ova, said in one case to owe their origin to a sexual process, and in the other cases to point to the existence of parthenogenesis among Bryozoa, under certain circumstances. Claparède has not confirmed the observations of Schneider on the development of Membranipora, but from what Nitzsche has observed of the early stages of Bugula, he appears to have seen the same retrograde development in the youngest stages of its larva which Schneider observed in Cyphonantes during its development into Membranipora.

The Coccoliths are Plants according to the recent inquiry of Mr. H. J. Carter, F.R.S. They are what Professor Huxley first thought them to be, not what he subsequently supposed in connection with his

Bathybius. Mr. Carter says, considering that the coccolith is so abundant in the Laminarian zone, and so voraciously fed on by the Echinodermata and Ascidiæ, also that it is so nearly allied to *Melobesia calcarea*, that it forms the bed of the Atlantic and is found fossilized in the chalk, he cannot help inferring that it is a vegetable organism which contributes chiefly to form the calcareous deposits of the present day as it has done in the past, at all events in the chalk.

A Mineral Silicate injecting Palæozoic Crinoids.—Dr. T. Sterry Hunt, F.R.S., states that a Silurian limestone from near Woodstock, New Brunswick, lately examined microscopically by Dr. Dawson, was found by him to consist almost wholly of comminuted organic remains, including fragments of trilobites, gasteropods, brachiopods, and joints of small encrinal stems and plates; the whole cemented by calcareous spar in a manner similar to many organic limestones. He observed, however, that the pores of the crinoidal remains were injected by a peculiar mineral, readily distinguishable in thin transparent sections, or on surfaces which had been exposed to the action of an acid, which dissolves the carbonate of lime and places in relief the injecting mineral. The minute structure thus revealed is precisely similar to that of recent crinoids studied by Carpenter, and will soon be described and figured by Dawson. Decalcified specimens exhibit a congeries of curved, branching and anastomosing cylindrical rods of the replacing mineral, sometimes forming a complex network, which under the microscope resemble the coralloidal forms of aragonite known as *flos ferri*, and present a frosted crystalline surface. The same mineral, as observed by Dr. Dawson, occasionally occupies larger interstices among the fragments, and was evidently deposited before the calcareous spar which cements the whole mass. When this limestone is dissolved in dilute hydrochloric acid, the residue, washed by decantation, equals from five to six per cent. of the weight of the mass, and is seen under a microscope to consist entirely of the casts composed of the mineral just noticed, mixed with about one-fourth of coarse silicious sand. This matter is pale greyish green in colour, but when calcined becomes of a bright reddish brown, without change of form. Heated in a close tube it gives off water, and becomes much darker in colour. It is partially attacked by strong hydrochloric acid, which takes up much protoxide of iron; but is readily and completely decomposed by hot concentrated sulphuric acid, leaving a skeleton of silica which, by a dilute solution of soda, is readily separated from the intermingled grains, more or less rounded, of colourless vitreous quartz.

The Mineralogy of Eozoon.—It is stated in 'Silliman's American Journal,' in a note, we think, by Professor Sterry Hunt, that Dr. Robert Hoffmann, of Prague, has submitted to chemical and mineralogical investigation the *Eozoon Canadense*, found at Raspenau in Bohemia.* He describes the Eozoon mass as having a superficial resemblance to that of Canada, appearing in waved or concentric bands, oval in form, or else in irregular acervuline aggregates. In the oval banded portion the shell of the Eozoon, a nearly pure, finely granular calcite,

* 'Jour. für prakt. Chemie,' May, 1869.

can be separated from the mineral representing the sarcode, which is described by Hoffmann as a cast of the soft parts of the Eozoon, formed through infiltration of watery solution either during the growth or immediately after the death of the animal. It is a peculiar silicate, fine-grained, greyish white, and somewhat translucent. Associated with this is a finely granular dolomite, destitute of any traces of organic structure, which sometimes appears to have served as a centre or point of attachment to the growing Eozoon. In other cases, however, broken fragments of older Eozoon had served as nuclei, and become surrounded with a fresh growth. These materials, which constitute what Hoffmann has described as the Eozoon reef, are associated with two other silicated minerals. One of these, allied to fahlunite, has a specific gravity of 2·687, is greyish brown or greenish black in colour, dull, or with a somewhat fatty lustre, and nearly opaque. This substance forms nearly parallel streaks in the central parts of the Eozoon reef, and moreover surrounds it, intersecting and wrapping around the Eozoon mass in multiplied layers, a line or more in thickness, which are interlaminated with a light green mineral, transparent, with a somewhat vitreous lustre, and a density of 2·56. It is a hydrous silicate allied to picrosmine, and is more or less penetrated by magnesite.

Observations on Surirella gemma.—Col. Woodward has published some recent observations on this species. The *Surirella gemma* has been recommended by Hartnack as a test for immersion objectives of high powers. Col. Woodward has not gained access to his original description, but finds accounts of his views, with figures, in the works of Drs. Carpenter and Frey.* Hartnack observed fine longitudinal striæ in addition to the fine transverse ones previously known to exist between the large transverse ribs; he supposed the true markings to have the form of elongated hexagons. Two handsome slides of this diatom were received at the Army Medical Museum a few months since, from Bourgogne, of Paris. A careful study of these by monochromatic sunlight inclines Col. Woodward to the opinion that Hartnack's interpretation is erroneous, and that the fine striæ are in reality rows of minute hemispherical bosses; from which, as in the case of other diatoms, the appearance of hexagons would readily result if the frustule was observed by an objective of inferior defining power to that he used, or if the illumination was unsuitable. This memorandum was accompanied by two photographs exhibiting what he saw; one was magnified 1034, the other 3100 diameters. The principal frustule shown in these photographs was $\frac{1}{290}$th of an inch in length. (The mean length of *S. gemma* is stated by the 'Microscopic Dictionary' at $\frac{1}{240}$th of an inch.) The fine transverse striæ counted longitudinally at the rate of 72 to the $\frac{1}{1000}$th of an inch. Transversely these were resolved into beaded appearances which counted laterally 84 to the $\frac{1}{1000}$th of an inch. If the structure consists, as he supposes it does, of fine hemispherical bosses, projecting from the surface of the frustules, the fact that these bosses are set

* 'The Microscope and its Revelations,' 4th edition, p. 182. 'Das Mikroskop,' 3rd edition, p. 40.

together more closely in the transverse direction than in the longitudinal would account for the elongated form of the pseudo-hexagons when seen. Some parts of the photographs closely approach Hartnack's description, but it is easy to observe that these are not the parts which are most nearly in focus. He has also resolved this diatom by monochromatic light derived from the electric lamp. The appearances obtained were identical with those above described.

Mr. Lowne on Pangenesis.—At a late meeting of the Quekett Club, Mr. Lowne, in making some observations on the white blood corpuscles of the newt, made some forcible remarks on the subject of *Pangenesis*. He recorded the experiment of Cohnheim on the blood corpuscle. If the swim bladder of a fish be filled with a solution of common salt, containing one per cent. of salt, tied and inserted under the skin of a rabbit or frog, after a short time, say twelve or twenty-four hours, it would be found filled by a large number of leucocytes, which had found their way into the bladder by permeating its walls. These were alive when they left the living tissue of the animal, but after going through the bladder their movements no longer continued, as they died in the saline solution; hence they were unable to escape from the bladder, and a large number became entrapped. This result not only applies to the swim bladder of the fish, for, according to Cohnheim, if the cornea of a frog's eye be taken (it must be quite fresh) and inserted under the skin of a living frog, in the course of twenty-four or forty-eight hours it will be found upon examination to contain a large number of leucocytes at various depths in the tissue of the cornea. Mr. Lowne considered these to be very remarkable properties, and he laid much stress upon them as throwing a great deal of light upon the doctrine of *Pangenesis*, as enunciated by Charles Darwin, by which doctrine it was supposed that particles or gemmules were given off from every part of the organism, capable of reproducing like parts under certain conditions, and of being collected in the *ovum*. The whole animal was thus permeated by particles passing off from the living tissue. It had been objected to by Dr. Lionel Beale that these particles, being solid, could not pass through the walls of a living cell; but if leucocytes could pass through solid tissues he could not see why minute gemmules, which might be solid or semi-solid, like leucocytes, should not pass through cell walls. He could not see why there should be any serious objection to the doctrine of *Pangenesis*. There was great difficulty in distinguishing a solid from a fluid. If the *Protozoa* be examined, many of them would be found to exhibit a series of gradations of solid matter, harder externally and softer internally; but no lines of demarcation could be drawn between the solid and more fluid parts of those animals, as one portion of the protoplasm shaded insensibly off into another.

The Darwinian Theory applied to Plants.—Certainly the best paper which has yet been published by the 'American Naturalist' is that in the July number on the above subject. It is of considerable length, abundantly supplied with notes, and is under the joint authorship of Dr. E. Muller and Professor F. Delpino. It has been translated into English by Mr. R. L. Packard, a gentleman who is himself familiar

to all the Natural History world. It is of such great length and so numerously annotated, that we cannot do more here than recommend it to the attention of our readers.

Scales of Butterflies and Gnats.—There would appear to be a singular resemblance between the scales of these two insect groups. In the above-mentioned paper the author says:—A fact casually discovered by me, and of which I find no mention hitherto, seems to me of great importance in the systematic disposition of this order. In the spring of 1868, while engaged in examining the head of a gnat, with a view to ascertain whether or not the valves of its proboscis had the transverse bands of chitine, I was surprised to discover that the proboscis and palpi were clothed with scales entirely like those of butterflies. I find no mention of this important fact in the special works of Meigen and Schiner which are in my possession. Meigen simply points out that in Culex, Anopheles, and Corethra, scaly productions are observed on the venation of the wings, and he figures some of them, which, however, being quite narrow and with two sharp points, have no analogy with real lepidopterous scales. The gnat-scales observed by me closely resemble the most characteristic lepidopterous scales. They suddenly dilate from a short and narrow peduncle to a large scutiform surface which is traversed longitudinally by a few parallel ridges, between which, when more highly magnified, transverse wavy lines, very fine and numerous, are seen. The only difference which these scales present compared with those of butterflies is, that in the former the transverse lines are not so fine, so regular, nor so regularly distributed over the whole surface; also these lines are entirely wanting upon the scales of some species of Tipulariæ. Finally, while the real lepidopterous scales are always deeply crenate at their truncated extremity, the scales of gnats are not; and their truncated extremity terminates in a very fine margin, from which the points of the longitudinal ribs sometimes project.

The Position of the Brachiopoda.—At a recent meeting of the Boston Society of Natural History, Professor Hyatt said his objections to Professor Morse's classification of Brachiopoda had heretofore rested wholly on the presumed affinities of the Polyzoa and Ascidia. He had been led by the similarities of the adult animals of the two groups to partially follow Professor Allman in his opinion that these two groups were closely related. In a paper "On the Fresh-water Polyzoa" * he had compared them, but had at the same time shown that the differences were much greater between the Polyzoa and Ascidia than between the former and the Brachiopods. Thus, there is no muscular system in the Ascidia which can compare in any sense with that of the Polyzoa; and in transforming the Polyzoon into an Ascidian, Professor Allman is obliged to violate this obvious difference, as well as to effect many changes which are not consistent with their organization. The nearer affinity of the Polyzoa and Brachiopoda is hardly questionable since the investigations of Koraleusky, who has shown us that the young Ascidians are apparently more like young vertebrata than they are like the young of the Polyzoa. The importance and value of the

* 'Proceedings [American] Essex Institute,' vol. iv.

resemblances existing between the adult Polyzoon and the adult Ascidian, so far as they may be supposed to indicate any close affinity or community of origin, are thus doubly denied by the differences of form and structure, both in the adults and in the larvæ. The Ascidians are also likely to be removed by these new discoveries, not only entirely away from the Polyzoa, but to an equal or greater distance from all the rest of the Mollusca; and even if we could in the face of embryology still maintain our comparison between the two structures, we should be contrasting the Polyzoa, not with a typical mollusk, but with an animal whose own position is very uncertain. He can think of no fundamental molluscan characteristics, either in the Brachiopods or Polyzoa, which ally them with the Lamellibranchs (clams), except those which join them still more closely to the Ascidians. Therefore, it seems clear, that if we separate the Ascidians from the Lamellibranchs, which they so closely resemble in their general adult characteristics, on account of their different developments, we must also, in turn, remove the Polyzoa from the Ascidians, and should logically regard the similarities of the two as analogies arising in different structures, and not as affinities derived from some common ancestor. Thus cut off from its quondam molluscan allies, our Polyzoon has but one refuge; its development points concisely to a vermian ancestor, and to this source we must relegate both it and its nearest ally, the Brachiopod.

Professor Agassiz's Future Dredging Operations.— Professor Agassiz has accepted an invitation extended to him by the American Coast Survey Bureau to take passage on the iron coast-survey steamer, which has just been built at Wilmington, Delaware, and which sails for the Pacific coast in September next. The expedition will take deep-sea soundings all the way, and extensive collections of specimens will be made for the Museum of Comparative Zoology at Cambridge. Secretary Boutwell has written to the Secretaries of State and Navy, asking that naval and other officers may be instructed to afford such courtesy and assistance to the exploring party as may be desirable. We learn also that Count Pourtales, of the Coast Survey, and Dr. Hill will accompany the expedition.

Structure of Stephanurus dentatus, or Sclerostoma pinguicola.— Dr. W. Fletcher gives a somewhat lengthy account of this worm. It appears not to have been correctly known in America till last year. A specimen was brought to Dr. Fletcher in 1866 by a farmer whose hogs were dying of cholera. He had removed the lungs of several, and also cut out fragments of the liver, all of which were spotted over with little cysts containing the worms; in the bronchial tubes down to the minutest branches, they were found in abundance and in situations where no one could have placed them. With these specimens his conclusion was that they were the *Filaria bronchialis* of Owen, or *Strongylus bronchialis* of Cobbold; and not having at this time made microscopic examination of this well-known kidney worm, the relationship between them did not occur to him at that time. In November, 1870, while demonstrating the portal circulation in the liver of a pig, full grown, he observed a worm which measured an inch and a half in

length, and in all respects resembled the kidney worm, and also reminded him of the worms he had examined five years before. Upon further dissection of the liver he found the worms not only free in the portal veins, but in cysts in various portions of the organ; also some were found in freshly-cut holes, directly across the hepatic lobules. The gall-bladder was distended with a dirty, yellowish fluid, the consistency of soft-boiled eggs, and although no worms were found, yet the ova were abundant, as they also were in the fluid of the cysts. Being convinced that the worm formerly examined in the lungs was the same as the worm now found in this new locality, and finding it oviparous, he gave up his opinion as to its being a *Filaria bronchialis*. From the date of this discovery, he frequented the slaughter-houses and pork-packing establishments, and found the worm in most instances in the pelvis of the kidney, or in cysts in the fat around them. Four times he has found the worm in the bronchial tubes, twice in the hepatic vein and in the right side of the heart; also in cysts throughout the fatty part of the animal. Frequently, when no worms were discovered, the eggs were abundant in the thick mucous-looking fluid in the pelvis of the kidney. This fluid contained, besides eggs, desquamated renal tubules, or casts and oily granules. In no instance has he found worms in an immature state, which shows that the eggs, in all probability, go through some other beast before they enter the swine, to become sexually mature. The symptoms in hogs which are referred to the "kidney worm," are due to a paralysis of motion in the hind legs; the hog drags the hind quarters along the ground from place to place in search of his food, although it is by no means proven that the worm is the real cause, unless some one is able to demonstrate its existence in some cerebro-spinal centre, or some point more likely to destroy the reflex power in the cord itself. The head and oral cavity are alike in male and female. The oral cavity is rather oval than round, and is surrounded by a hexagonal frame, each corner having a papilla and hooklet, while each side is armed with six serrate teeth. Looking into the oral cavity, it is funnel-shaped, having three openings at the back, one of which connects directly with the œsophagus, while the others appear to connect with the water vessels. The intestine is long and contains some pigment granules, arranged in dendritic forms, throughout its length; the whole is thrown into convolutions, and gives an almost black appearance to the worm, except when the white oviducts distended with eggs, or the seminal vessels of the male are folded over the intestine, when it has a white, mottled appearance. The caudal extremity of the female is spindle-shaped, but has two little bursæ higher up. In the male it is formed by three-lobed bursæ, above which are two well-developed flexible spicula.—*The American Journal of Science and Art*, June, 1871.

The Gregarinida and their Development.—M. Edouard Van Beneden has very fully worked out this subject in advance, one may say, of Lieberkühn, and the other observers who have devoted themselves to it. Indeed in the case of *Gregarina gigantea*, with which the present essay is almost alone concerned, he has watched all its successive

transformations, from the time of its being a small protoplasmic mass sprung from the *Psorospermia*, until its completion was arrived at, till in fact it attained 16 mm. in length. He has found in the lobster's intestine multitudes of small protoplasmic masses resembling the *Protamœba primitiva* of Haeckel, with certain distinctions from it however. They are distinguished from the true Amœba by the absence of a nucleus and a contractile vacuole. These have no projections from them. There are, however, others which have one, or more frequently two prolongations in the form of arms, which M. Van Beneden says resemble the mobile stem of Noctiluca, and these forms he calls generating *cytodes*. He then describes the movements of them, one of the arms or projections of which is motionless. After a time the other increases in length till at last it breaks away, and having specially an undulating motion, it is like a nematoid worm. Curiously enough, when the movable arm has been discharged, the further development of the arm at rest begins, and it goes through the same process of development and motion as the preceding, with this difference, that instead of becoming detached from the central mass it gradually absorbs it as a vertebrate embryo absorbs the contents of the vitelline sac. The resemblance of the animal thus formed to the Nematodes has led the author to style them *pseudofilariæ*. He then proceeds to describe the further development of those peculiar bodies into *Gregarina gigantea*. After this comes a discussion of various questions connected with the subject. These are as follows: "Monera and the monerien phase of the Gregarinidæ," in this Dr. Beales' germinal matter theory is discussed; "The evolution of Gregarinidæ put in the light of an example of endogenous generation;" "Importance of the nucleolus;" "Are the gregarines a regressive development of the Amœba?" and lastly, "Do the gregarines present an alternate generation?" Thus concludes a memoir of nearly forty pages, and with an excellent plate. We think it is one of the most important papers that have appeared for some years, and we commend it to our readers.

NOTES AND MEMORANDA.

How to close Cells filled with Glycerine.—This important subject, and one which too little is generally known about, was discussed lately before the Quekett Club.* The question was asked by Mr. Henry Lee, as to how cells were filled with glycerine, so as to prevent the glycerine after a while from escaping. Mr. Suffolk said that he was one of those who had been extremely successful in keeping glycerine in. His plan was as follows:—When the cell was closed he varnished it with a coating of common liquid glue, and when this was dry he put it under the tap, and thoroughly washed it, in order to remove any glycerine which might remain outside. After carefully drying the slide with blotting paper, he gave it another coating of the liquid glue, and when dry repeated the washing process, and after having

* Journal of the Club, July.

given it a third coating in the same manner, he gave it a final coat of gold size, and he had never had any trouble with cells closed in this manner. He believed that the secret of success in a great measure was owing to the washing; the gold size was also removed from contact with the glycerine by the elastic varnish under it. Glycerine would do no harm to gold size when it could not get at it. Subsequently, at the Secretary's request, Mr. Hislop said that his experience was that of Mr. Suffolk, for he had found no difficulty in keeping in glycerine. His plan was to make a good seat for the cover, first by a thick ring of gum dammar, allow this to become sticky, and then put in the glycerine, lay on the cover, and then carefully wash off all superfluous glycerine. When perfectly washed and dried, put on two or three coats of gum dammar to finish it. He had in this way mounted slides which had kept well for more than two years, and he strongly recommended gum dammar for the purpose. Care must be taken that the glass was perfectly clean, and if this were attended to, and a good bed was made of cement on which to place the cover, there would be no doubt as to the result. He had some large preparations —such as a whole frog or toad passing from the tadpole condition— which had been put up in this way, and they were perfectly intact, and not the slightest exudation had ever been observed. Finally, Dr. Matthews mentioned that it was from Mr. Hislop that he derived the idea of making a ring upon the slide; in doing this it was necessary to wait before putting on the cover until the gum dammar became "tacky," because if this were not done it would be found that tears of dammar would run in and spoil at least the appearance of the slide around the edge. If, after making the ring, the slide were put aside for an hour before proceeding to mount, there would be no danger of this occurrence.

A New Sub-stage for a 4-inch Objective.—At the meeting of the Quekett Club on May 26th, Mr. James Smith exhibited a simple contrivance designed to obviate a difficulty frequently met with in using Ross's 4-inch objective, in consequence of the great length of focus required. It consisted of a small mahogany sub-stage, attached to the wooden stand of the microscope, and jointed so that it could be set at any angle required to make it parallel with the ordinary stage when the body of the instrument was inclined. The object to be examined was placed upon the sub-stage, and viewed through the orifice in the upper stage, by which means an ample length of focus and a great degree of steadiness were obtained. For the illumination of opaque objects in this position, the concave mirror was admirably adapted, and the lamp did not in that case require to be placed so near to the instrument as to cause any inconvenient amount of heat to the observer. The habits of living insects could be most advantageously studied by this arrangement; and he had recently observed, when examining a spider, that, in attacking a fly, and enveloping it in a quantity of web previously to finishing his meal, a silken thread was spun from each of the five spinnarets, instead of from one only, as under ordinary circumstances.

CORRESPONDENCE.

On some New Parasites.

To the Editor of the 'Monthly Microscopical Journal.'

July 8, 1871.

Dear Sir,—May I be permitted to draw attention to some parasites named and figured in the July number of the 'Monthly Microscopical Journal,' by Mr. T. Graham Ponton, F.Z.S.

Having been engaged in collecting and studying these insects for some years, I am well aware of the difficulty of making out the genera and species from dried or mounted specimens, and of the consequent necessity of carefully exact drawing in the illustration of these minute forms.

Without entering upon the many structural inaccuracies with which Mr. Ponton's plate is literally crowded,* I wish simply to notice two errors, which, if uncorrected, are enough to throw the whole subject into confusion.

In Fig. 3, a presumed new species of the sub-genus Docophorus, the legs end in single claws,—whereas all the species of Docophorus and the allied sub-genera have a pair of claws proceeding from their short two-jointed tarsi.

The Trichodectes, Fig. 4, is figured with double claws; the chief character of the genus being that they are single. In fact, all the species of the obsolete order "Anoplura," which infest mammals, have single tarsal claws.

A Trichodectes with double claws is as absurd to anyone familiar with the genus as a horse with cloven feet.

I remain, dear Sir, yours faithfully,

H. C. Richter.

The New Metallic Covered Glass Chimney.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Some short time since it occurred to me that a very useful addition to our microscopic appliances might be made by having the ordinary glass chimney *silvered on the outside*, and covered with some protective substance.

I had one made on this principle, and showed it to Mr. Swift,

* [It is almost unnecessary to observe that the author is alone to blame. Proofs of the plate were sent to him, and his corrections were carefully carried out: the original drawings were such as to render the artist's task one of no small difficulty; but for the points referred to by Mr. Richter, Mr. Ponton is solely to blame.—Ed. 'M. M. J.']

of University Street, who suggested that a covering of copper would be the best protection.

It will be seen by this arrangement that the whole of the chimney is covered with the exception of a small space through which the light passes, so that it acts as an ordinary chimney-silver-reflector and lamp shade, and in one.

Thus saving much time, trouble, expense, and space.

It is supplied by Mr. Swift at the trifling cost of fifteen pence.

I remain, yours &c.,

FREDERICK BLANKLEY.

HOW TO SELECT SPECTACLES FOR NEAR-SIGHT AND FAR-SIGHT.

ROCHEFORT-SUR-MER.

SIR,—I take the liberty of sending you a note, which you will please insert in the 'Monthly Microscopical Journal,' if you think it suitable.

I am, Sir, yours,

MOUCHET.

Note concerning certain Micrographical Measures, and containing some information useful to persons suffering from Myopia and Presbyopia.

I do not know the instrument employed for measuring the thickness of the thin glass plates used for covering microscopic objects, nor do I know if there is one for valuing the relief of those objects. In every case, I use my microscope for both purposes, and also for pointing out to sufferers from Myopia and Presbyopia the glasses suitable to their sight; and the object of the present note is to describe the process that I employ.

In every microscope of a certain price there is, besides the tube which is worked by friction, or by a rack movement, or by both, a micrometric screw for finishing the focussing, that is, one with a very fine movement, and consequently possessing great delicacy. It is important to have this screw made with great care. This screw is what I use for measuring the thickness of covering glasses, and of microscopical objects. For this purpose I have fixed on its head, which, as in most instruments of this kind, is placed in the upper part of the tube,* a dial divided into 100 parts, and high enough above the *moletée* part not to impede the movement of the fingers, when focussing. On the *canon* which supports the tube is a ring, slit like the *canon* itself, and kept in its place by its own elasticity. This ring carries an index opposite the dial, and can be turned so as to bring back the

* In this respect differing from English instruments.

index, before the commencement of a measurement, exactly opposite a division of the scale; in this way there are no fractions to be noted down at the starting point, but only at the end of the operation.

A turn of the micrometric screw of my microscope being equal to half a millimètre, each division of the scale, in consequence of the whole being divided into 100 parts, is equivalent to $\frac{1}{200}$ of a millimètre. In order to measure the thickness of a round thin glass cell cover for example, I begin by drawing with a diamond a very small stroke on the edge of one of its surfaces. I make a similar mark on the opposite surface, in such a way that the two marks may form a cross; this is in order not to confound them. If the glass is square, the cross will fit much more easily in one of the angles. One of the marks (the upper one) is then brought into focus, and the division of the scale opposite the index noted down. The body of the microscope is then moved down by turning the head of the screw from left to right, until the second mark comes into focus, while the first disappears. I now find out the thickness of the glass by ascertaining how many divisions of the scale have been passed over, counting from the starting point already noted. As regards microscopic objects, however thin they may be, provided they have two faces distinct enough, I bring one face into focus as starting point, then examine the scale of the screw, and proceed as before described. I measure in this way the thickness of *diatomaceæ*, those interesting little beings with silicious envelopes, whose *striæ*, to use the old term, are not in the same focus. When I wish to show a preparation to anyone not much accustomed to use a microscope, this process enables me to regulate the focussing for other objects also, after I have once determined the difference between our sights, by means of an examination, made by us both, of the first preparation. I can also determine with much greater facility whether the focus of both eyes differs in either of us, and in what proportion.

I come naturally to spectacles next. When the sight is not in its normal state, biconvex and biconcave lenses, with both faces similarly curved, are usually employed to remedy imperfections arising from defective refrangibility of the eye, or from disturbance in its accommodating power. Lenses of those kinds are the most powerful, their construction is the simplest, and the focal distance is most easily calculated, as it is equal to the radius. Each of the two surfaces of those lenses is a segment of a sphere of known diameter. The shorter it is, the greater is the convexity or concavity.

Medium sight is estimated at 10 English inches, or 25 centimètres. By experience, I find my own to be 30 inches, or 75 centimètres. Those both being known serve as the basis of the following calculation. I multiply 10 inches (the number in normal or medium sight) by 30 (that is my own), and find as result the number 300, which, when divided by the difference, that is by 20, gives 15 inches as quotient. This quotient indicates both the focal distance of the lens suitable for my sight, and the radius of the sphere, which is the same thing. We speak of inches, because the lenses of spectacles are still marked according to the old measure. This method of measuring the focal

distances of lenses, pointed out by Messrs. McKenzie, Wharton Jones, and Michael Levy, is, according to M. Desmarres, not perfect, but I believe that it may be corrected by the microscope.

The value of each division of the dial of the micrometric screw already mentioned, must now be ascertained in the special case which occupies us. There is a difference of 20 inches between normal vision and mine. I assume that it may be represented on the dial by ten divisions, each of which will be equal to 2 inches: $10 \times 2 = 20$. Let us take an example. I wish to find out for a *presbyope* the lens suitable for his sight. I begin by putting in focus an object, which for this purpose will serve as a type. I next observe on what division the index is, and then cause the dial to turn *ten divisions* from left to right (from right to left when the patient is myopic) *in order to bring it to normal sight.* This being done, I ask the person in question in his turn to bring the object into focus. If we find that there is between the two sights a difference of eleven divisions on the dial, each of which is equal to 2 inches, we have 22 inches as the result. This we multiply by 10 (the number of inches for normal sight), and obtain the number 220, which, when divided by the difference, that is by 12, gives as quotient 18 inches (and a fraction which may be omitted) as the focal distance of the lens, or the radius of curvature.

The operation is evidently simple and easy. Unfortunately, opticians do not all mark their spectacle lenses according to a uniform system of numbers. Some numbers indeed indicate the focal distance or radius of curvature, but others indicate the diameter of the sphere on which the lenses are formed. Other numbers, again, belong to series peculiar to certain manufacturers, and do not afford the least information. There are even a great many lenses not marked with any number. Hence arises an amount of confusion, very perplexing to those who are compelled to have recourse to spectacles to aid the eyes, in their efforts at accommodation, at short distances. We know at least through the foregoing operation, when the two surfaces have the same curvature, the focal distance of the lenses suitable for our sight, and we can select them ourselves at the optician's, or better still at home, if he will intrust them to us, by means of the following very simple process. One of the lenses is exposed to the solar rays in such a position that its axis is directed towards the sun, that is, that the plane of the circle bounding the lens is perpendicular to the rays. If the lens is convex, and a screen is held parallel to this plane, the image of the sun is seen to form a circle on the screen. This circle varies in size according to the distance between the lens and screen, and the screen must now be moved farther or nearer, without destroying its parallelism, until the point is found at which the circle appears smallest and brightest. The screen is then at the focus, occupied by the little luminous circle, and the distance from the lens to the focus can be easily measured.

It should be remarked that the numbers of spectacle lenses indicating the focus, either for myopian or presbyopian sight, are not consecutive. Naturally that one must be selected which approaches nearest to the figure given by the operation; and although this operation may

be performed without the microscope, it is evident that far more satisfactory results will be arrived at with the aid of this instrument. Such is my conviction.

MOUCHET.

[M. Mouchet's letter contains some peculiar expressions which have rendered its translation by no means an easy task. We trust, however, that it will be intelligible to our readers.—ED. 'M. M. J.']

PROCEEDINGS OF SOCIETIES.*

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

June 8th. Ordinary Meeting. Mr. T. H. Hennah, Vice-President, in the chair.

Mr. Wonfor announced the receipt of fourteen volumes of works on Natural History, from Mr. T. Davidson, F.R.S., the Proceedings of the Maidstone and Mid-Kent Natural History Society, and the three last papers of the Eastbourne Natural History Society from the Secretaries. Votes of thanks were passed to the donors.

Mr. Wonfor announced the discovery of a human skeleton at a depth of five feet in the works of the Intercepting Sewer, opposite Brunswick Terrace, and reported on the success of the Field Excursion of June 3rd to Plashett Park. Votes of thanks were passed to Lord Gage for granting permission to visit Plashett, and to Mr. Grantham for piloting the Society on the occasion.

Mr. Robertson exhibited a specimen of *Sepiola oceanica*.

Mr. Perley presented a water-colour drawing for the Society's album, from a sketch made near Staplefield, at the Annual Excursion last year, for which a vote of thanks was given.

Mr. G. Scott then read a very able and interesting paper "On Rude Flint Implements." The immediate occasion of the paper was the receipt of a parcel of such implements from D. Stevens, of St. Mary Bourne, by the Hon. Secretary, who had handed them over to Mr. Scott to bring the subject before the Society for discussion.

June 22nd. Microscopical Meeting. Mr. T. H. Hennah, Vice-President, in the chair.

Mr. Wonfor announced that one of the honorary members, Mr. T. Curties, of Holborn, had kindly sent for exhibition a large number of slides of hairs and scales of plants, from his private collection, as his

* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'

contribution towards the evening's business, with a promise that at all times he should be pleased to help in the way of specimens.

In the course of a discussion on the nature and uses of hairs and scales in the economy of plants, Mr. Hennah mentioned that most plants possessing hairs, if properly manipulated, showed the circulation or rotation of the cell contents, a state of things admirably seen also in the rootlets of the frog-bit *Hydrocharis morsus-ranæ*, and suggested, as the Society, on the occasion of its annual excursion to Arundel, would be in a district especially rich in pond life, that, if the next meeting were without subject, many interesting things might be obtained for exhibition.

Mr. Wonfor considered the suggestion a very good one, because, in addition to Arundel, they would, before their next Microscopical Meeting, visit Plumpton, a locality very fertile in pond life.

The meeting then became a conversazione, when

Mr. Hennah exhibited the specimens lent by Mr. Curties, the most noticeable among which were hairs of different species of *Alyssum*, *Tillandsia zonata*, *Loasa*, *Onosma taurica*, and sections of the stems of different *Solanaceæ*, &c.

Mr. R. Glaisyer exhibited some very beautiful scales of different ferns *in situ* and as transparent objects, and the silicious scales of *Deutzia scabra* and *gracilis*.

Mr. Wonfor exhibited various scales and hairs, chiefly of a stellate character, some of which, from different ferns, such as *Niphobolus bastata* and *lingua*, the stag's horn *Acrostichum alcicorne* and *Rhododendron ferruginum* were much admired, as were also some of the same scales seen under polarized light. Later in the evening he showed the rotation of the cell contents in the beaded hairs in the stamens of Tradescantia, the blue spider wort.

Bristol Microscopical Society.

At a meeting of this Society held the 18th of May last, being the first held since January in consequence of the removal of the collections from the Bristol Museum, in which building the Society meets, Mr. R. M. Bernard, President, occupied the chair.

Mr. T. G. Ponton, F.Z.S., read a paper "On some rare Parasites, together with a Description of Four New Species."

Reading Microscopical Society.*

April 4th, 1871.—Mr. J. C. Simpson read a paper "On a Simple Method of Producing the Fibrillation of Albumen: and can Coagulation and Fibrillation be Considered Electrical Phenomena?"

The former was thus described:—Place some egg-albumen on a glass slide, touch a thin glass cover with a speck of nitric acid, bring it down on the albumen, and place the slide under the microscope. A whitish opaque spot is at once seen, and this the microscope resolves

* Report furnished by Mr. B. J. Austin.

into the appearance of fine granular matter, exactly resembling albumen coagulated by heat. The outward diffusion of the acid may then be detected, under a high power, by the gradual formation of a cloud of fine particles and by a peculiar faint line which marks the boundary of the acid. At the same time masses of fine fibre are formed in the central white granular matter, and between it and the boundary of the acid ring, but never beyond.

This method of producing coagulation and fibrillation, and the fact of their production by a current of electricity, led the writer to consider the question as to whether or not the formation was a *purely* electrical phenomenon.

The evidence adduced—to show that coagulation and fibrillation were merely chemical phenomena, and that, though produced by a current of electricity, they were but secondary effects of that current, due to the decomposition of the fluid—was,

1stly. That the arrangement of the coagulated or fibrillated matter is not in accordance with electrical laws, which it should be if an electrical phenomenon. The writer stated that by the aid of the galvanometer a current of electricity could be detected on the addition of acid to albumen; but argued that there was no necessary connection between the current and the fibrillation, as the union of acids and alkalis always produces a current, whether the liquid be coagulable or not.

2ndly. That if coagulation and fibrillation were electrical, then such phenomena, when produced by other means than electricity or acid, ought to give rise to an electrical current. The writer stated that he had made careful experiments with the galvanometer and found that such was not the case.

3rdly. That if coagulation and fibrillation were purely electrical, then such phenomena (and especially fibrillation, which is a continuous act of growth), taking place within the magnetic field, ought to produce the fibrillated matter in some definite direction according to the lines of magnetic force. The writer stated that he had performed experiments by means of an electro-magnet, and under the microscope, but could not perceive that the arrangement of the fibrillated matter was at all influenced by magnetism, and hence concluded that these phenomena could not be regarded as, in any sense, electrical.

Mr. Tatem exhibited a series of tests of *Difflugia acanthophora* from Bulmershe Lake, and lately, for the first time in this neighbourhood, detected there. It appears to be a very variable species; not only were the examples shown more globular than *D. acanthophora* is described to be and figured by Pritchard, but the spines vary also very much in number, direction, and position. The testa may have three, four, five, two outcurved spines, a single stout, straight central spine, or may be altogether spineless, in which last condition the species has improperly received the name of *D. areolata*.

May 2nd.—Mr. Tatem, in the absence of Dr. Shettle, and at his request, read an extract from the 'Athenæum' of April 1st, 1871, which stated that M. Becquerel, sen., had laid on the table of the Academy of Sciences, of Paris, a MS. work, in which the author had

demonstrated, amongst other things, "the influence of electric action in the transformation of the blood, in the body, from venous to arterial," and explained also, "by electric currents, an action which chemistry has been unable to account for, that is to say, the transport of materials within the organism, that is to say life, for life resides in movement."

Mr. Tatem exhibited mounts of diatoms from Duckpond Falls, and the Secretary exhibited *Antennaria (?) semiovata*, a minute fungus.

Microscopical Society of Liverpool.

The third meeting of the Session was held at the Royal Institution, on Friday, 3rd March, the Rev. W. Banister, B.A., President, in the chair.

Mr. J. H. Day made a donation of twelve slides of seeds, and Capt. J. A. Perry of a large collection of dredgings, insects, &c., from the Brazils; the thanks of the Society were voted to the donors.

Capt. Perry exhibited a mechanical turn-table, devised and constructed by him on his homeward voyage; in this ingenious little instrument the revolution of the table is effected by clockwork, and is controlled by a catch regulated by the finger of the operator.

Mr. W. Wood exhibited a microscope lamp made by Messrs. Abraham and Co.

Mr. Thomas Higgin sent for exhibition a number of photographs of diatoms and other test-objects by Lieut.-Col. Woodward.

Mr. T. J. Moore exhibited some young shrimps hatched that day in the Museum.

Mr. G. F. Chantrell exhibited and illustrated by a diagram a singular confervoid growth that had made its appearance in a glass cell in which he had kept rotifers for upwards of three months: the rotifers attached themselves to the filaments and became encysted.

Mr. A. C. Cole exhibited a new diatom which he had found in the Nottingham (Maryland) earth, and mounted with his usual perfection. The diatom is not only new as a species, but in all probability is entirely unknown, certainly unnamed, generically. It combines some of the characteristics of Aulacodiscus and Triceratium, but is evidently very distinct from either. In form it is a hexagon, with the sides arched inwards; has a finely-developed convex umbilicus, from which proceed radial lines terminating in club-shaped bosses, which do not reach the border. The position of these lines may be understood by supposing a letter X to be laid horizontally upon a letter I, so that the intersection of the X may coincide with the centre of the I.

Three other lines proceed from alternate angles of the hexagon, but are not continued to the centre.

The surface of valve when viewed with the $\frac{1}{4}$th or $\frac{1}{8}$th objective appears beautifully watered.

The Rev. W. H. Dallinger called the attention of the Society to the fact that Mr. H. J. Carter had found that the coccoliths of Huxley were abundant in the Laminarian zone of the Devonshire coast, and after careful study had determined them to be vegetable organisms

entirely and independent of the so-called Bathybius, being indeed calcareous algæ.

The coccospheres were considered by Mr. Carter to be the sporangia of the above genus.

The proceedings concluded with the usual conversazione.

South London Microscopical and Natural History Club.*

A meeting was held on Saturday, March 11th, at Glo'ster Hall, Brixton Road, for the purpose of organizing a club for microscopical research, combined with the study of natural history. Dr. Braithwaite, F.L.S., F.R.M.S., &c., presided.

Mr. Hovenden, F.A.S.L., acted as Secretary for the evening. The Chairman, in stating the object of the proposed society, remarked that Mr. Hovenden was the real originator of the club. This gentleman thought that a society of the kind would find a sufficient number of supporters in the district, and he therefore communicated with several gentlemen well known in the microscopical world, whose names were eventually appended to a circular letter which was issued for the purpose of calling together the present meeting. Dr. Braithwaite then spoke of the advantages of the microscope in investigation, and of the usefulness of a club for the purpose of diffusing microscopic knowledge, collecting objects, &c. By comprehending natural history in their study, they would include the two great sections, botany and micro-zoology; in this way the younger members would be instructed in natural history.

Mr. Stephenson proposed "That a society be and is hereby formed, to be called the South London Microscopical and Natural History Club."

Mr. Stewart seconded this motion, which was carried unanimously.

Mr. Henry Lee (of Croydon), F.L.S., F.G.S., said that he was glad to see that the number of microscopical societies was fast increasing. He mentioned that within the last few months three new societies had been formed; one at Forest Hill, numbering forty-four members, one at Margate, of thirty-three members, and now this one in South London which he hoped to see the most important of all. He then complimented the Society upon the number of eminent names that were down on the subscription list, and was glad to see that they were all thorough good working members. He recommended to the Society a systematic mode of working, the subject of the next meeting always being announced, as desultory work seldom led to good results. He thought that each member should bring his microscope, and mentioned that at Croydon a rule was made that the microscopes of all the members should be at the disposal of the lecturer for the evening. Respecting the subscription, he thought that the experience of all other microscopical societies taught that it should not be a less amount than 10*s*. This sum excludes nobody, while a smaller sum will not allow the club to carry on its business. He also mentioned that the club at Croydon had been invited by Mr. Saunders of the Holmsdale Club to assist them in work-

* Report furnished by the Secretary.

ing up the fauna and flora of the county of Surrey, and he advised the new club also to take part in this. Mr. Lee concluded by saying that he did not agree with some who said that these local clubs interfered with the Royal and other large societies, but rather believed that many were induced to join the large societies by first becoming members of the local clubs.

Mr. Deane, F.L.S., F.R.M.S., quite coincided with the views expressed so ably by Mr. Lee, and in confirmation thereof recapitulated his experience since first he became a founder of the "Royal" Society. He believed that these societies did a vast amount of good, by bringing together young and old, to unite in one common fellowship of love. On his first joining the "Royal," he was delighted to see the strong tendency such societies had to break up all feelings of clique among scientific men. All were put on an equality, each vying with the other in the acquirement of microscopical knowledge. He was certain that those who took any interest in microscopical study were better members of society in consequence; their minds began to expand; they would find that the microscope in one hour's labour preached a sermon to them that would last through the whole of the day; and this influence would be felt all through life. He therefore hailed this Society as one calculated to do an immense amount of good in the district.

Mr. Soper recommended excursions on the principle of the Holmsdale Club.

Mr. Jackson moved "That the subscription of the club be 10*s.* per annum, payable in advance."

Mr. Deane seconded the motion, which was carried unanimously.

Mr. Ackland moved the election of Mr. Deane as provisional President.

This motion was seconded by Mr. Rutter, and carried unanimously.

Mr. Deane, in accepting temporarily the post of President, said that he quite approved of the suggestions that had been put forward, as to excursions, and although he feared that he should be unable personally to take part in them, he felt sure that they would be found of great importance in the club.

Motions were then passed appointing (provisionally) Dr. Braithwaite and Mr. Stephenson as Vice-Presidents; Messrs. Ackland, How, Jackson, Neighbour, Rogers, Stuart, and Suffolk, as Committee; Mr. Robinson as Treasurer; and Mr. Hovenden as Honorary Secretary.

It was also proposed and carried unanimously, "That a list of subscribers be opened," and "That the Committee be requested to frame suitable rules, and present them for approval at the next meeting."

It was arranged that the next meeting should be held on Saturday April 1st, at 8 o'clock in the evening, at the same place (Glo'ster Hall), when the recommendation of the Committee as to the future place of meeting will be laid before the members. The meeting then separated, thirty-eight names having been placed on the subscription list as members of the club.

The first Ordinary Meeting of this Club was held on Saturday,

April 1st, at Glo'ster Hall, Glo'ster Place, Brixton Road; Mr. Deane, F.L.S., F.R.M.S., presided.

The minutes of the inaugural meeting were read and confirmed.

Mr. Hovenden, F.A.S.L., the Honorary Secretary, read the report of the Provisional Committee, which was as follows:—

"Your Committee beg to report that in accordance with the resolution of the club, made on the 11th ult., they have carefully framed and considered a set of Rules for the guidance of the club, which are this evening placed before you for your consideration and approval.

"On the inaugural meeting thirty-six gentlemen subscribed their names as members, and since that time thirty-two have been added, thus making the number of members up to this evening sixty-eight.

"Your Committee beg to recommend that on this the first ordinary meeting of the club, the subscription list be kept open, but that hereafter all new members shall be balloted for in accordance with the 4th Rule.

"And, having carefully considered the place for future meetings, recommend that for the present the club meet at Glo'ster Hall, Glo'ster Place, Brixton Road."

Mr. T. Sebastian Davis moved, "That the report be received, adopted, and entered upon the minutes."

This motion was seconded by Mr. Reynolds, and carried unanimously.

The Rules drawn up by the Committee next occupied the attention of the meeting, and, as all present had received copies of these Rules, it was agreed that they should be taken as read.

Mr. Davis said that he was sure that all present would agree with him that the Committee had done their work in a very satisfactory manner. There was one thing that he, as a member of the Council of the Photographic Society, wished to mention, and that was, that if the meetings of the club were held on the second Tuesday in each month, as proposed, many members of the Photographic Society would be unable to attend, as this was the day fixed for their meetings. He therefore proposed that the meetings of the club should be held on the third Tuesday in each month, instead of the second.

This proposition having been agreed to,

Mr. Davis then moved, "That the Rules proposed by the Provisional Committee be adopted, with the alteration, that the meetings be held on the third Tuesday, instead of the second Tuesday, as provided therein."

Mr. Hardess seconded this motion, which was carried unanimously.

Mr. Izard proposed, "That the provisional officers of the club be elected permanent officers for the ensuing year, with the addition of the following gentlemen:—Mr. James M. Cottrell, Dr. Hector Helsham, Dr. N. W. Ord, to complete the members of the Committee, according to Rule 2."

This motion was seconded by Mr. West, and carried unanimously.

It was arranged that the next meeting of the club should be held on Tuesday, April 18th, at half-past seven o'clock in the evening, Mr. Suffolk having kindly consented to read a paper on this date.

The meeting then resolved itself into a conversazione. And it is evident that a society of the kind has been much needed in the neighbourhood, as so many microscopes were brought by the members of the club that it was impossible to exhibit all. Twenty-five of these instruments were, however, shown, the various objects being viewed with much interest by the members and visitors present, who did not separate until a late hour.

In the course of the evening fifty-six additional names were entered upon the books of the Society; thus making a total, since the formation of the club, of ninety-two members.

An Ordinary Meeting of this Society was held on Tuesday, May 16th, at Glo'ster Hall, Glo'ster Place, Brixton Road; Henry Deane, Esq., F.L.S., &c., in the chair.

The minutes of the last meeting were read and confirmed. Seventeen new members, whose certificates had been read at the last meeting, were balloted for, and elected unanimously. The certificates of thirteen new members were also read.

Dr. Robert Braithwaite then read a paper "On the objects of the Society, and the best methods of promoting them." We are sorry to be unable to give the whole of this interesting paper, of which the following is an abstract:—

In speaking of the objects to be carried out by associations like the present Society, it is scarcely necessary to dwell upon the advantages resulting from an acquaintance with science and natural history, for it must be profitable to everyone to know something of the structure and functions of the organic world around us, and especially in the young is it laudable to foster those habits of observation and orderly arrangement which hereafter will be of service in every walk of life. And, since we must presume that those who know most of God's works are indeed living nearer to himself, can we believe that he would have granted to man the skill and thought required to construct this glorious instrument, the microscope, placing him at the gates of a new wonderland, and then that its exploring powers should not be used to the uttermost?

And this leads me at once to what I should wish especially to see cultivated, the natural productions of the district, the vegetation of the soil, and the living things, particularly the insects, which are found there also; and I trust an herbarium and cabinet in which these may be preserved will at no distant date form an important feature in the objects of the club.

Biology, or the study of living things, naturally divides itself into two parts, the animal and vegetable kingdoms. Taking a rapid survey of the vegetable kingdom, we place the *Algæ*, or water-weeds at the bottom of the scale, together with the minute forms (*Diatoms* and *Desmids*) found in our ponds. Following these come the *Fungi*, and above these the *Lichens* and *Mosses*. Next we place the *Ferns*. These form the *Cryptogamous*, or non-flowering plants. From these we pass to the *Phænogamous*, or flowering plants, in which pollen is produced. As well-known objects for the microscope, I may just refer to the pollen, to the cuticle, or skin covering the leaves, with its

stomata, or breathing pores, and various forms of hair, and to the modifications we may notice in divers seeds. Passing next to the animal kingdom, we find the lowest animals, like the lowest plants, are of the simplest structure, as a type of which I may mention *Amœba*, a little gelatinous speck we find gliding on the surface of ponds. After this we get species invested in a thin flexible shell, and so we pass on to those beautiful perforated organisms dredged up from the ocean bed. Next come the *Sponges*, and after these we have the great group of *Infusoria*, the animalcules *par excellence*, for some of them are the most minute of living creatures, all of them interesting to watch when living, but unfortunately not capable of being preserved satisfactorily. The great division of *Articulata* embraces animals whose limbs are composed of jointed segments, and organs in lateral pairs. Lowest of these, and passing as it were from the last group, are the Worms, which have of late acquired considerable importance, from the mode in which even human beings may be infested by them,—I refer to the *Trichina spiralis*, that minute worm which, sometimes by millions, inhabits the muscles of animals used for our food, and which thus get introduced into the human economy. The *Rotifera* include many beautiful microscopic animals; there is still much to be learned respecting their anatomy and life-history. Above these comes the group comprising the Crab, Shrimp, and Lobster, as well as many minute forms which abound in our ponds. Following these is the group comprising the *Spiders* and *Mites*. Both are especially worth investigation. We now arrive at the great class of insects, the study of which, or *Entomology*, has now become most ardently pursued. As objects of interest, even to the young, few surpass a well-mounted collection of insects; and I do look forward to the time, I trust not far distant, when the insect fauna of the district shall be represented in the cabinets of this club by actual named specimens. The last great division of the invertebrate animals are the *Mollusca* or Slugs and Shell Fish, but of these time does not permit me to speak. I may at least mention the names of the vertebrate animals, these being Fishes, Amphibia, Reptiles, Birds, Mammalia, and by these we reach the prince and head of all creation—Man. And what does the microscope teach us with regard to ourselves? Not that we stand apart from the rest of the organic world, but that of the many thousands of living beings, each forms a link in the vast chain by which even Man and the Amœba form one harmonious whole. To man, however, is given something more—knowledge, reason, and consequently responsibility, and by these we are led far beyond the limits of the small circle in which we travel, still to find ever outreaching the farthest grasp of instrumental power, yet present every moment to our unaided vision, one Creator and Preserver of all, by whom, and through whom, we, and all things, live, and move, and have our being.

A vote of thanks was unanimously accorded to Dr. Braithwaite for his interesting paper.

The Chairman announced two excursions—one to Elstree, on May 27th; and another to Homerton (for Hackney Marshes) on June 10th.

The meeting then resolved itself into a conversazione, when many interesting microscopical objects were shown, after examination of which the members and visitors separated, the next meeting being announced for Tuesday, June 20th, at half-past seven o'clock in the evening.

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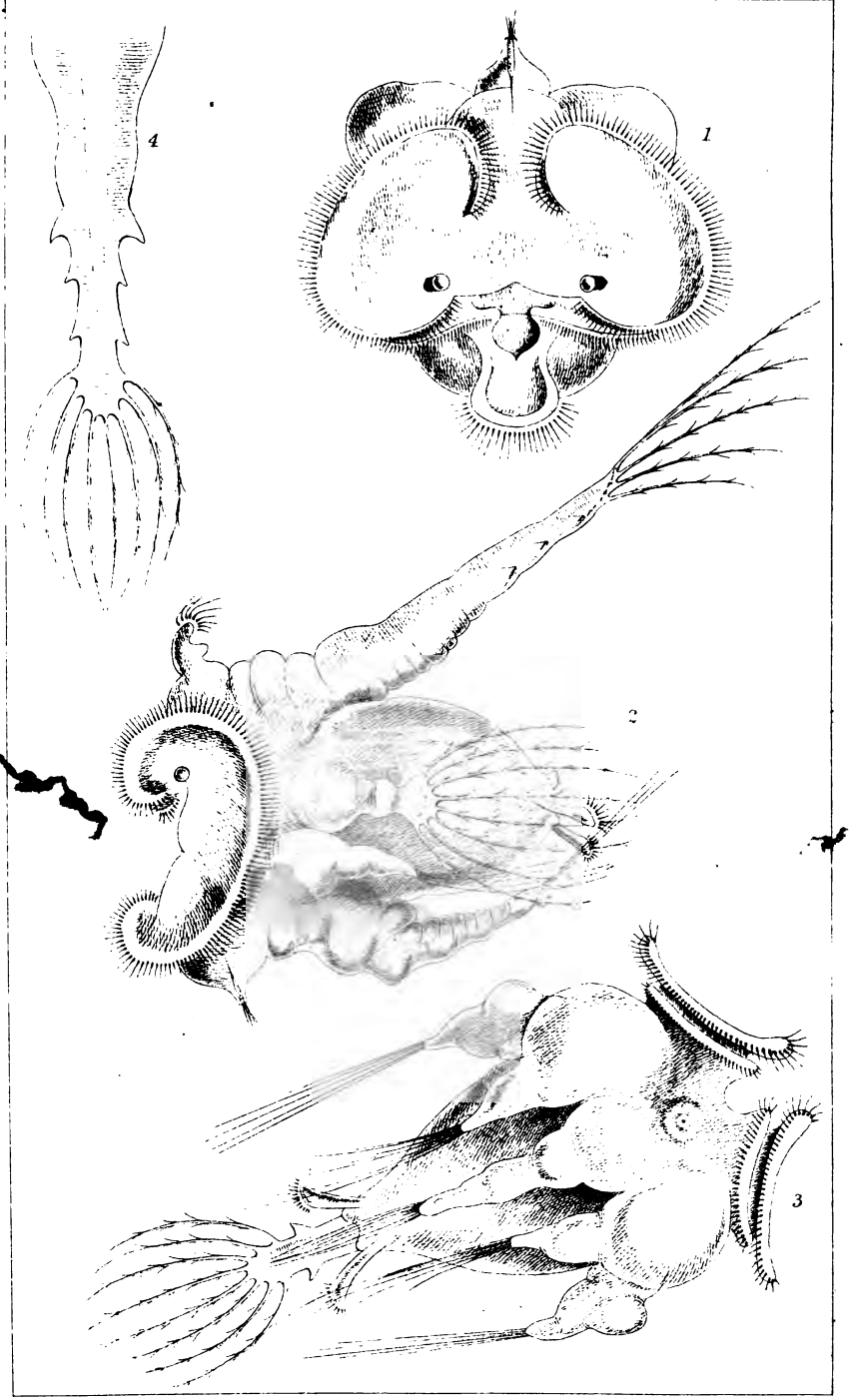


Dr Hudson.

Pedalion mira, Anew Rotifer.

THE

MONTHLY MICROSCOPICAL JOURNAL.

SEPTEMBER 1, 1871.

I.—*On a New Rotifer.* By C. T. Hudson, LL.D.

Plate XCIV.

A rotifer hunter should never pass a pond without trying it, no matter how often he has been disappointed before. For example, here is a pond close to my house, which, so far as I can remember, has never yielded anything worth catching for many summers, and yet this July I have taken in it in one dip several specimens of *Synchæta*, *Asplanchna*, *Brachionus*, and *Anuræa*, as well as of a new and very extraordinary rotifer.

On hunting over the bottle with a hand lens before taking it home, I readily recognized the first four, while the latter I supposed to be some new and unusually large species of *Polyarthra*; but on placing a specimen under the microscope I for a moment doubted whether it was a rotifer at all, and not a larva of some one of the *Entomostraca*. A brief examination showed the animal to be a true rotifer with a splendid trochal disk and ciliated chin, and with internal organs much like those of *Triarthra*, but with an external form of a most unusual character; for it possesses six well-defined limbs containing powerful muscles, and terminated not by cilia but by fan-shaped plumes of fringed hairs, and is in these respects so utterly unlike any other rotifer, that, though it has many points of resemblance to the *Hydatinæa*, I am quite puzzled where to place it.

I propose to call it *Pedalion mira*, from the oar-shaped limb with which it steers its way like an ancient trireme; occasionally however improving on its antique type by striking a succession of vigorous slaps with all its limbs in concert, and darting through the water with such speed as to clear quite sixty times its own length

EXPLANATION OF PLATE XCIV.

Fig. 1.—Front view, showing the dorsal antenna, eyes, double row of cilia round the trochal disk, and the mouth lying between them and above the ciliated chin.

„ 2.—Side view.

„ 3.—Dorsal view.

„ 4.—Pseudopodium; ventral surface.

All the figures are on the same scale, and represent a magnification of about 300 linear.

in a second, that is to say, at a rate which in a man would be upwards of 200 miles an hour.

Every new rotifer is a new difficulty. *Hydatina* is troublesome enough, for it is never still, and will not bear compression. *Synchæta* has in addition the charming habit of turning over head and heels. *Triarthra* and its small cousin *Polyarthra* by help of their spines and plumes add skipping to their other accomplishments, and were, till I found *Pedalion*, the most vexatious of all the rotifers.

But while *they* are chary of using their full powers—reserving them for great occasions, such as when they fall foul of each other or run against the sides of their prison—*Pedalion* skips habitually without the slightest provocation, and at tolerably regular intervals, so as to drive an observer into a fit of nervous irritation.

Now it is hopeless to attempt to make out its shape or internal structure unless the creature can be kept tolerably still, and yet I found it impossible to hit upon any way of ensuring this at my own pleasure. If I secured *Pedalion* between the two plates of the compressorium, it was only to see its limbs reversed and thrown into unnatural attitudes, hiding often the very things I wanted to look at. If I placed it in a very tiny drop of water and gave it just room to swim in, before many minutes had elapsed it either jerked itself half out of the drop and lay sprawling, helpless, and disfigured; or with its back adhering to the concave boundary of the water it kept "squirming around" (as a young American lady described it to me) like a horse in a circus.

It was clear that, unless the rotifer would of its own accord sit for its portrait, there was no Plate to be had for the 'Microscopical Journal,' so I abandoned the heroic methods and tried coaxing and patience.

I first placed one or two scraps of conferva on the under-glass of the compressorium so as to cross each other, and then I went to my tank in which the rotifers were, and, watching till the currents that are always rising and falling in it, had swept into one corner a tolerably dense crowd of them, I drew up a score or two with a pipette, and dropped them with as little water as possible on the interlacing weed.

On placing the cover over them, and gently compressing the weed, I had of course many rotifers entrapped in small live cages deep enough for them to swim in pretty freely without getting far out of range. It was a pretty sight to see them darting about, diving, skipping, and rolling in every direction; but I began to fear that my scheme would fail, for I seemed to have made them more restless than ever. After a while, however, they seemed to get tired, or to be reconciled to their prison, and then one or two settled down to exploring the confervæ, sailing along with their mouths and

chins close to the stems. At last one got its head into the angle where two stems met, so as to prevent its swimming farther, and, to my delight, made no attempt to flap itself away in the usual fashion. I at once slipped a circle of ruled glass into the eye-piece, seized my paper, ruled also with squares, and had the good fortune to complete the side view given in Fig. 2 before Pedalion stirred; when it did go, it went with one bound utterly out of the field.

The sitting being at an end I had leisure to consider my sketch, and once again I could scarcely credit what I had seen and drawn. Here was a rotifer with six good-sized limbs all worked by muscles contained within them, and terminating in fans of long rigid fringed hairs, more like the extremities of a *Daphnia* than those of the rotifers. Many rotifers have one limb—the pseudopodium—which is attached somewhere or other to the ventral surface, and is in *Pedalion* the grand oar, acting as flapper and rudder, which springs from under the chin. *Synchæta*, too, has a small movable projection at each side of its head bearing its ciliary paddles; but where are the links that connect *Synchæta's* rudimentary stumps with *Pedalion's* well-formed limbs? It is very possible that such links exist, and may yet be discovered; for the world of the free-swimming rotifers is a comparatively unexplored one still, and everyone who ventures into it is pretty sure to find something new, even if he cannot hope to rival the achievements of Ehrenberg, Leydig, or Gosse.

I once caught at Guernsey, in the upper mill-pond at Petit Bot, some rotifers whose ciliated disks were of the shape of the top joint of a thumb, and were densely covered on their ventral surfaces with fine cilia, like fur, leaving bare only a straight pathway, as it were, through the cilia, in a line with the animal's length.

I am perfectly certain that I never saw the creature before, and I can discover no account of anything like it in the text-books. Unfortunately I delayed one day before I examined and sketched my captives, and on the next they were dead. I returned to Petit Bot for a fresh supply, and found that the miller had let out the water to clean the pond.

I have dwelt on Pedalion's external form, partly because I have not yet had time to thoroughly examine its internal organs, but mainly because this rotifer appears to be a weighty witness on the side of those (to wit, Owen, Leydig, Gosse, &c.) who would range the Rotifers rather with the Crustacea than with the Annelida, &c. One of the arguments of Vogt (as quoted by Pritchard) against the Crustacean nature of the Rotifers is that "a pair of jointed locomotive organs is never found among the Rotifers at any period of their existence." Now, as Pedalion has *two* pairs of such organs, besides its pseudopodium and another limb on the dorsal surface, the above statement must be modified, and the argument will in consequence be considerably weakened.

Into the question of the affinities of the Rotifers I do not intend to plunge. I shall consult my own tastes and capacities better by hunting for a four-legged rotifer, now that I have found a six-legged one. At some future time I hope to return to the examination of Pedalion's structure. Its length is about $\frac{1}{120}$th of an inch from the front of the trochal disk to the extremities of two curious ciliated projections at the posterior end of its body.

II.—*On the Examination of Mixed Colouring Matters with the Spectrum Microscope.* By H. C. Sorby, F.R.S., &c.

In studying the colouring matters soluble in water that may be obtained from various kinds of *Algæ*, for which special names have been proposed, as though they were single and simple substances, I have been led to conclude that they are in some cases mixtures of at least four, which are readily distinguished by their spectra. The facts which have thus presented themselves have so impressed me with their importance in such inquiries, that I think it may be well to make the study of mixed colouring matters the subject of a special communication. The interest of this question, in connection with certain branches of botany, will, I trust, be made fully apparent when I have shown that various kinds of marine and fresh-water *Algæ*, and even some Lichens, though differing very much in general tint, may contain one or more particular colouring matter in common, and differ in the presence or absence of others. There is thus a bond of union between somewhat remote members of certain natural orders, whereas without proper attention each of these mixed colours might be thought to be a special kind, and no such connection between the plants would be recognized.

The manner in which the mixed nature of some colouring matters may be ascertained from their spectra has been already described by Professor Stokes* and others, as well as in previous papers by myself; but in order to make this communication complete in itself, I must be allowed to again describe some of them. I shall not attempt to enter into the chemical part of the subject, or to treat of the separation of different substances by purely chemical methods, such as the solubility or insolubility in various reagents, but confine myself almost entirely to those processes in which the examination of the spectra is of primary importance. I scarcely need say that the coloured material should be separated, as far as can be conveniently managed, into that which is soluble or insoluble in such simple solvents as water or alcohol; but at the same time there are cases in which such a difference in solubility does not appear sufficient

* 'Journal of the Chemical Society,' June 2, 1864 (New Ser.), ii., pp. 304–318.

to prove that the colouring matter itself differs essentially. The spectra seem to show that occasionally the presence of some other substance, insoluble in water, which has a strong affinity for the colouring matter, is the true cause of this variation. I shall therefore presume that we have to deal with colouring matters separated from any others that differ materially in their solubility.

There are few cases in which the mixed nature of a coloured aqueous solution can be more easily ascertained than when the constituents differ so much in character that the addition of some reagent will more or less completely destroy the spectrum of one without having any effect on that of the other. For this purpose no substance is superior to sulphite of soda. Without producing any real decomposition, this almost entirely removes the detached absorption at the red end of the spectrum of certain colours, but has no effect whatever on that of others. In the case of some colours it thus acts when the solution contains excess of ammonia, but in the case of others it has then little or no action, but removes the absorption when the solution contains excess of such a moderately weak acid as citric. The application of this principle to the examination of the colouring matters of plants is extremely simple and satisfactory. If, for example, we obtain the red colouring matter from the leaf-stalks of the common red rhubarb, or from the petals of the crimson *Calceolaria*, as described in my paper "On the Colouring Matter of Leaves,"* and to its solution in water add a little citric acid, the colour is clear, deep pink, made almost or quite colourless by the addition of a little sulphite of soda, which change in colour is due to the complete removal of the broad absorption-band in the green. If, however, this same red colouring matter is mixed with some of the yellow substances found in many leaves, the spectrum of the said solution, in addition to the same broad absorption-band in the green, shows absorption at the blue end; and on adding the sulphite, this yellow colour, which absorbs the blue end, may be distinctly seen alone. A similar plan may be adopted in the case of some substances which are changed when the solution contains slight excess of ammonia. I could not give a more striking example than that of a solution of blood mixed with so much magenta that the absorption-bands of the blood are completely hidden. On adding a small quantity of sulphite, they are seen as well as if no impurity had been present. It may, however, happen that there is a mixture of two colours, both of which are acted upon in the same manner by the sulphite, but one more rapidly than the other. Thus, for example, the colouring matter of the petals of the blue *Lobelia* gives a spectrum with two remarkably distinct absorption-bands. So also does that of the petals of the

* 'Quarterly Journal of Microscopical Science' (New Ser.), xi., 1871, pp. 215–234.

crimson *Cineraria*, but the bands are so placed that on mixing the two colours a band of one covers up the clear space between the bands of the other, and thus the mixture may show merely a broad absorption, very like that in the colouring matter of many plants. However, on adding a little sulphite of soda to a very slightly acid mixed solution, and examining it at once, the bands of the *Cineraria* may be seen for a short time, since those of the *Lobelia* are more rapidly destroyed. Even in the case of those colouring matters which are not immediately changed in an acid solution by the addition of sulphite, one may slowly fade, whilst the other is scarcely changed, and thus evidence may be obtained of the existence of a mixture. Numberless illustrations might be given of the application of such a method, but these will I trust explain sufficiently well the general principle. In particular instances we may of course make use of other reagents to destroy one colour and leave another, but in the case of closely-allied substances the difficulty is that few or no reagents will act sufficiently on one without changing both, and we are compelled to rely on the comparison of the spectra of the colours partially separated artificially, or met with naturally in varying proportion.

For effecting a partial separation of colouring matters soluble in water no reagent is more convenient than ether. On agitating the aqueous solution with more of this than can be dissolved, the excess rises to the top, and the aqueous solution subsides to the bottom. In the case of some colouring matters the greater part remains dissolved in the water; whereas, in the case of others, the greater part rises to the top, dissolved in the ether; whilst occasionally nearly the same amount is dissolved in both. It will thus be seen that, if a mixture were thus treated, a partial separation might often be effected, and on evaporating to dryness, redissolving in water, and comparing the spectra, either in the natural state or after reagents had been added, the differences might clearly prove that two or more colouring matters were present. In order to make this more intelligible, it will be well to enter into a few general principles involved in the method. For example, suppose that in some particular state, acid, neutral or alkaline, whichever it might be, the original solution gave a spectrum with two absorption-bands, A and B, and that when separated artificially into two portions one gave the band A and the other B, in precisely the same conditions as before, it would be quite certain that the original was a mixture of two substances. But since such a complete separation would only occur in a few instances, it might happen that both the products showed both the bands, only in a very different degree. Thus, for example, on making the solutions of such a strength that the band A was equal in both, the band B might be so much darker in one, that in experiment tubes of the same depth the solution might have to be diluted to four times the volume

before it gave that band no darker than in the other solution. Expressing this by figures in front of the letters, we may say that the spectra were as follows:—

The original solution	A + B
The part soluble in ether	2A + B
The part soluble in water	A + 2B

We have here very good proof of the existence of two substances. If there had been only one, however much we might have divided it into different portions, the relative intensity of the two bands would have remained the same, that is to say, in solutions of equal strength, in all the above cases we should have had simply A + B. Of course the extent of this separation might be more or less than what I have supposed, but on detecting any such difference we should endeavour to effect a still further separation, and may perhaps ultimately succeed in obtaining both colouring matters in a comparatively pure state. The same principles are equally applicable in the case of substances insoluble in water, but soluble in alcohol, which can be more or less completely separated by the use of bisulphide of carbon. For example, I found that on agitating an alcoholic solution of chlorophyll from green holly leaves with excess of bisulphide of carbon, the part carried down in solution in the bisulphide did not give the same spectrum as that left in the alcohol. Making certain absorption-bands seen at the blue end, equal in both cases, other bands at the red end were so much darker in the part carried down in the bisulphide, that the solution had to be diluted about eight times, before they became equal. There being thus evidence of a mixture, I tried the experiment over and over again, in order to ascertain whether a more complete separation could be effected, and found that, if the leaves were well crushed and then heated in alcohol, the solution rapidly filtered, and treated with the bisulphide as soon as cold, it could be separated into a deep green, carried down by the bisulphide, and an almost clean yellow, left in the alcohol, which corresponded to the xanthophyll of yellow leaves. The separation was indeed so complete, that for an equal amount of xanthophyll this yellow solution did not contain above $\frac{1}{30}$th the amount of chlorophyll contained in the other solution, so that a mixture of these two colours in ordinary green leaves was placed beyond all doubt, as also shown by Professor Stokes in the paper already cited.

Such a case as this leads me to the consideration of the spectra of substances giving several absorption-bands. For example, suppose the original solution showed four bands, A, B, C, and D, and when partially separated, as described above, the spectra became as follows,—

Original solution	A + B + C + D
One product	2A + 2B + C + D
Another	A + B + 2C + 2D

We might conclude that very probably two substances were present, one giving the bands A and B, and the other C and D. Similar principles would of course apply to cases where more or fewer bands were present. Very often one colouring matter might give well-marked bands, and the other only a general absorption. We might then separate it with two products, and on making the solution of these of the same general depth of colour, one might show a spectrum with well-marked bands, and the other scarcely any trace of them. The same general facts may be seen when the characteristic bands are only developed by the addition of different reagents, which completely alter the colouring matter, but then more care is required to avoid any differences that may result from the varying action of more or less of the reagents. To attempt to describe all the necessary precautions would make this paper most unreasonably long, and therefore I trust that the above rough and general account will at all events suffice to point out the sort of plan that may be followed, premising that in all cases the results should be checked by other experiments, in order that no accidental peculiarities may lead to error. As illustrations of the application of these principles I would refer to those colouring matters of *kino*, and of *Gummi rubra*, which are soluble both in ether and in water. The *kino* contains two such, one more soluble in ether, of pink colour, which in its natural state gives a well-marked absorption-band between the yellow and green, and the other, more soluble in water, of orange colour, which gives a broad absorption-band between the green and blue on the addition of bicarbonate of ammonia. *Gummi rubra* contains this same orange colour, without the pink, mixed with another orange or yellow substance, relatively more soluble in water, not yielding a spectrum with decided bands in any state that I have yet seen.

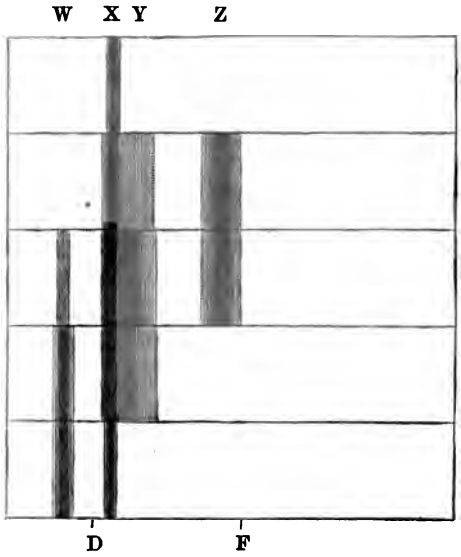
When a solution contains more than two colouring matters the recognition of each becomes somewhat more difficult; but still, by following out this system, and dividing the material into more than two portions, a very good opinion may be often formed of the general optical properties of each substance. When some of them give well-marked and characteristic absorption-bands, and when the absorption of others may be removed by the addition of sulphite of soda, the study of a complex mixture is very greatly facilitated, and especially if the spectrum of one or more of the constituents is of such a marked character that it can be at once recognized as that of some substance already known in a pure state. A tolerably good opinion may then be formed of the spectrum of the rest, by, as it were, subtracting that of the known constituent. This leads me to the description of the spectra of certain colouring matters which are met with so far separated naturally that their compound

character may be inferred without reasonable doubt, and confirmed by a more extended examination.

I have lately found that many interesting facts may be observed by examining the spectra of substances in their natural state, without extracting the colouring matter. Frequently they are so opaque that it is requisite to use a very bright light to penetrate through them. I employ, as a condenser, a plano-convex lens about ·7 inch in diameter, and of about the same focal length, over which is placed a conical cap with a round hole a little larger than the field of the microscope as seen through the slit in the spectrum eye-piece, at such a distance from the lens within the focal point that the image of the sun illuminates the entire hole. By placing the object on a piece of thin glass, and bringing this hole close up to it, sufficient light may be driven through a comparatively opaque object, and none which has not penetrated through it can pass up the microscope, even when the specimen is small. In this manner the petals of flowers may be examined direct; and if the colour is too pale with one petal, two or more may be pressed flat, one over the other. We may thus readily see two well-marked absorption-bands in the blue in the light transmitted through the petals of several species of *Brassica*, of *Helianthemum vulgare*, *Geum urbanum*, and several other yellow flowers; but in the case of many others, as *Viola lutea* or *Ranunculus bulbosus*, though similar bands are seen in exactly the same part of the spectrum, they are so much more faint on account of general absorption at the whole of the blue end, that there must certainly be a second yellow colour present. Some yellow flowers, like *Chelidonium majus*, give well-marked absorption-bands in such a different position as clearly to show that they are due to another substance; but on the whole the spectra of very many can be readily explained only by supposing that they are coloured by a very variable mixture of at least two yellow substances. The facts seen in the case of the spectra of various *Algæ* are, however, so much more striking, that it appears to me that I could not possibly choose a better illustration, and that a somewhat detailed description will serve well to explain the method which I think should be followed in such inquiries.

The following woodcut shows the spectra of the light transmitted by the various plants themselves, so far as relates to the colouring matters soluble in water. I have left out all such absorption as is due to chlorophyll and xanthophyll, for the sake of simplicity. I may here remark that it is most important to study the spectra of the plants themselves, since in some cases the colouring matters are decomposed so rapidly in solution that they are almost or entirely lost, and new absorption-bands are developed, due to products which do not occur in the living plants.

DIAGRAM OF THE SPECTRA OF THE LIGHT TRANSMITTED THROUGH VARIOUS ALGÆ.



1. *Palmella cruenta.*
2. Red *Floridea.*
3. Purple *Floridea.*
4. *Oscillatoria* grown in water.
5. *Oscillatoria* (Microcoleus?) grown on a damp wall.

(Fraunhofer's lines.)

1. *Palmella cruenta* is a microscopic plant, closely allied to the so-called "red snow," found growing over the surface at the bottom of damp walls, and looking as if blood had been spilt on the ground. The spectrum may be seen by scraping off the external red part, and keeping it for a while damp on thin glass, so that the red granules may extend over the vacant spaces. It is, however, still better to keep a quantity of the impure mass quite wet in a bottle, when in the course of time the comparatively pure red material will grow up the surface of the glass, and may be collected together and examined on a piece of thin glass with concentrated sunlight. The spectrum is characterized by the single well-marked band X. When the plant is kept in dilute bicarbonate of ammonia it is killed, and the colour dissolved. It is a fine pink by transmitted light, and gives the same absorption-band as seen in examining the plant itself. This substance is extremely fluorescent, of orange colour, giving a spectrum with a remarkably bright green-yellow band, lying just on the red side of the band X in the transmitted light, and not coinciding with it, as has been inaccurately said to be the rule in such cases. This remarkably bright green-yellow band of the fluorescence enables us to recognize this substance, even when mixed with so much impurity that the absorption-band of the transmitted light is almost or entirely hid.

2. What I have named red *Floridea* is one of those normally purple Algæ which appear to turn red in a manner similar to the

leaves of many terrestrial plants in autumn.* The chlorophyll has disappeared, and perhaps more xanthophyll has been formed. However, confining our attention to the colouring matters soluble in water, it will be seen that the band X is visible, but that instead of being isolated as in the spectrum of *Palmella cruenta*, there is a broad absorption-band Y extending from it some distance towards the blue end, and also another well-marked band Z, between the green and the blue.

3. The purple *Floridea* is a similar plant to the red, only in an unchanged condition. The spectrum shows a band W in the red, due to a blue colouring matter, which along with the chlorophyll causes the plant to be of a peculiar dull purple colour, and not red, as in the former example. It will be seen that the band Z is the same as in the red specimen, but that Y is relatively so much darker that the narrow band X is almost entirely obscured, and yet that it does really occur is proved by the bright green-yellow band in the spectrum of the fluorescent light of the colouring matter dissolved out by water.

4. The spectrum of *Oscillatoriæ* is seen to the greatest advantage by examining a mass of the healthy living fronds which creep up the sides of a bottle containing the impure natural material. They are then free from *Diatomaceæ* and particles of mud, and by collecting them together on a piece of thin glass with a little water, so that there are no vacant spaces to signify, concentrated sunlight penetrates *through* them, and gives a spectrum with the two well-marked absorption-bands, shown in the figure, as described by Ray Lankester.† When the plant is of that kind which forms a dark velvety mass on the surface of shady troughs supplied with water from cold springs, we have the well-marked band W, and the broad band Y, which is so dark that it almost hides the narrow band X, and thus, as far as that part of the spectrum is concerned, it corresponds with the purple *Floridea*, but differs in the absence of the broad band Z. The presence of the substance which gives the narrow band X is, however, proved by the spectrum of the light of fluorescence.

5. The *Oscillatoria* which grows on damp walls and ground is perhaps not the same plant as that growing under water, and may belong to the genus *Microcoleus* of some authors. The spectrum of the pure living plant, separated as described above, shows the band W, and also the narrow band X, without the broad band Y, seen in the case of *Oscillatoria* grown under water. Much more might be said about the changes that occur at different seasons of

* See my paper "On the Various Tints of Autumnal Foliage," 'Quart. Journ. of Science' (New Ser.), i., 1871, pp. 64–77; and that in the 'Quart. Journ. of Micros. Science' already cited.

† 'Monthly Micros. Journ.,' iv., 1870, pp. 14–17.

the year; but since I am only using these facts as illustrations of another subject, and am not writing a paper on the colouring matters of Algæ, I wish to confine myself to what is requisite on the present occasion. What I therefore now contend is that a simple comparison of these five spectra shows that they are due to the variable admixture of four different colouring matters, which, for convenience, I will call W, X, Y, and Z, from their respective absorption-bands, as shown in the woodcut. In the first place there can be no doubt that there is a simple substance X which gives rise to the single band seen in spectrum No. 1. This same band occurs in No. 5, but along with it is another, due to a blue-purple colour W, which is obtained separate in solution by keeping the plant for some time in a little water. The spectrum of the light transmitted by this solution shows the single band W and that of the splendid rose-coloured fluorescence a single rose-coloured band, just on the red side of the absorption-band. As will be seen on comparing the spectra, No. 4 differs from No. 5 in having a broad absorption-band Y. That both the substances W and X are really present is shown by the spectrum of the fluorescence of the aqueous solution obtained from that kind of *Oscillatoria*, which shows both the narrow rose-coloured and the narrow green-yellow bands, and thus, though I have not yet been able to procure the substance Y separate from all others, I do not hesitate to conclude that it does exist as a third independent colouring matter. That there is a fourth, giving rise to the band Z, is also clearly shown by comparing either spectra Nos. 1 and 2, or 3 and 4, and this conclusion is established by the fact that, on keeping for a few days the mixed colouring matters dissolved by water from the purple *Floridea*, and removing the deposit by filtration, a flesh-coloured solution may be obtained, which gives a spectrum with this single band Z.

According to these principles the colouring matter of the various plants may be expressed as follows:—

1. *Palmella cruenta*	X
2. Red *Floridea*	X + Y + Z
3. Purple *Floridea*	W + X + Y + Z
4. *Oscillatoria* grown in water	W + X + Y
5. *Microcoleus* grown on a damp wall	W + X

The substance described by Kützing, Stokes, and Askenasy by the name *Phycoerythrin* appears to have been a mixture similar to 2 or 3, along with perhaps another substance, which I have not yet seen. Cohn's *Phycocyan* must, I think, have been a mixture of W, X, and Y with another, which was possibly an altered product, giving an absorption-band not seen in the spectrum of the plants themselves. I may here say that I have met with at least four such products, and one, which gives a narrow band in the red, is

formed so readily that it is sometimes almost impossible to obtain an aqueous solution of the mixed colours free from it.

All these substances belong to a single natural group, distinguished by certain marked characters, and since the mixture of several of them has been named *Phycoerythrin*, it may be well to call it the *Phycoerythrin* group. It differs entirely from the *Erythrophyll* group, described in my paper "On the Colouring Matter of Leaves," in the fact that the position of the bands is very little, if at all, changed by the addition of weak acids or alkalies; but when they are stronger, actual decomposition occurs, some of them being more easily changed by acids and some by alkalies. The difference in the spectra shown in the woodcut cannot therefore be caused by any variation in the acid reaction of the juice of the plants, as is the case in the colouring matter of the petals of some flowers. The possibility of this must always be borne in mind in applying such a method of comparison, and the effect of various reagents must always be ascertained before the actual identity of the colouring matters can be inferred from the agreement in the bands in only one particular condition. Much may be learned by acting directly on the plants themselves.

The consideration of the various spectra described above seems to lead to the following conclusions:—

1. When a spectrum shows two absorption-bands, like No. 5, they should not be considered due to one single substance until satisfactory evidence of the fact has been obtained. The solution should be allowed to undergo slow decomposition, and be repeatedly examined, in order to ascertain whether both bands disappear in the same proportion, and also the action of various reagents observed, in order to learn whether one band can be permanently removed without the other, making of course due allowance for any change that may depend merely on an acid or alkaline reaction.

2. When more than two bands are seen in the spectrum, as in No. 3, and they are not at nearly equal intervals, the compound nature of the substance may be considered so probable, that further examination should certainly not be neglected.

3. When there is broad shading about a narrow absorption-band, as in No. 2, it is important to ascertain whether or not it is due to the same substance. There are certainly many cases in which I have always concluded that both are due to the same, but examples like this evidently show that such an opinion ought not to be formed until after further examination.

The occurrence of so many associated colouring matters, as in Algæ, may be rare. It must not be supposed that I imagine that whenever there are two or more absorption-bands they are due to two or more independent substances. As an example of what I look upon as satisfactory proof of the contrary, I will describe some

facts connected with the well-known spectrum of blood. If after exposure in a dry state to the air for some weeks, until the hæmoglobin has been changed into methæmoglobin, a small quantity of the double tartrate of potash and soda be added to the aqueous solution, and afterwards a very minute portion of the double sulphate of protoxide of iron and ammonia, the methæmoglobin is deoxidized and reconverted into hæmoglobin, as described in my late paper "On Blood-stains."* Here then we have a decomposition gradually effected by the atmosphere, and if two different substances had been present, it is extremely probable that they would have varied in the rate of change, so that there would finally have been an alteration in their relative proportion, and thus when deoxidized there would not have been the same relation between the absorption-bands as in fresh blood. I find, however, that the agreement is complete. Moreover, if the colouring matter had been a mixture of two substances, it is extremely probable that there would have been some such variation in their relative amount in the blood of very different animals, as occurs in the colouring matters of different Algæ. In order to ascertain whether this is the case, I carefully compared side by side the spectra of human blood and that of the small annelids so common in stagnant pools, and found that the position and relative intensity of the two bands was exactly the same.

Such then are the principal conclusions that have been forced on my attention in carrying out these investigations. For my own part I must say that they make me think that many of my previous observations require further examination, in order to ascertain whether I have not sometimes believed that I was examining a single substance when it was really a mixture. For the future I shall certainly be quite alive to the importance of the principles described in this paper, and trust that what I have said will serve to impress it on others, and assist them in carrying out similar inquiries.

* 'Monthly Micros. Journ.,' vi., 1871, pp. 9-17.

FIG. I.

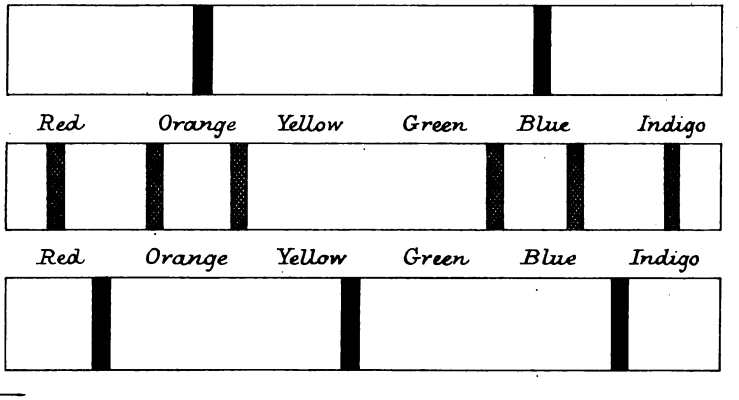


FIG. II.

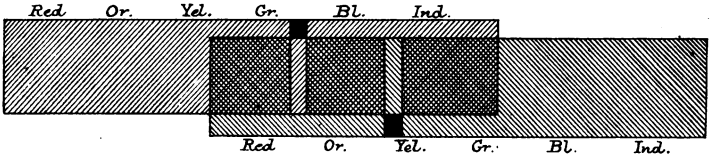


FIG. III.

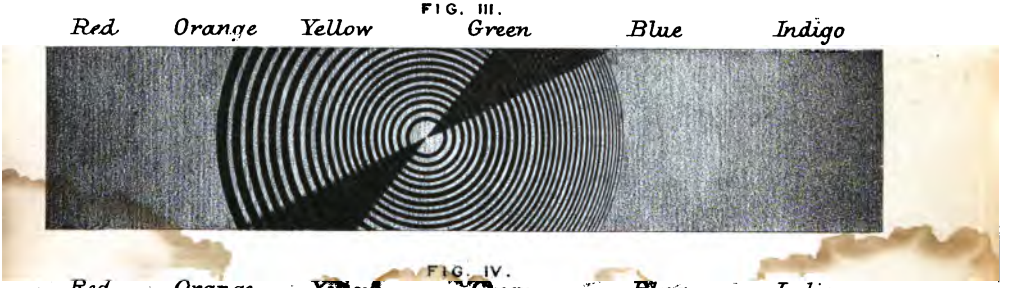
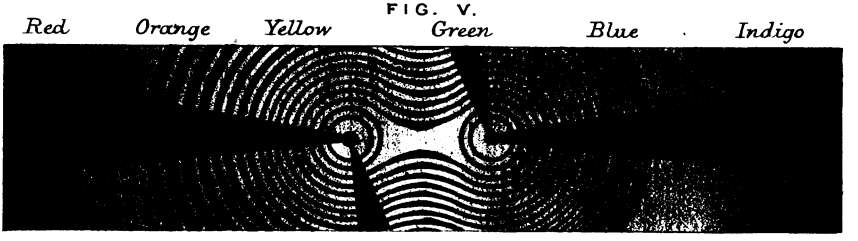


FIG. IV.

FIG. V.



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Spectra by polarised light through doubly-refracting crystals.

III.—*On Spectra formed by the passage of Polarized Light through Double-refracting Crystals seen with the Microscope.*

By Francis Deas, M.A., LL.B., F.R.S.E.

Plate XCV.

It is familiarly known as one of the commonest experiments in optics that when a beam of polarized light is passed through a thin film of mica or selenite, and subsequently analyzed either by reflexion or by double refraction, two colours are seen complementary to one another, and alternating with one another at each 90° of a revolution of the analyzing plate or prism.

It might be expected that the coloured light thus obtained would, if thrown into the form of a spectrum by means of dispersion prisms, exhibit some peculiarities, and such is the case, as will be seen from the following experiments:—

To make the experiments intelligible, it may be well in the first place to say a few words about the instrument employed, and the method of using it.

Any spectrum microscope ought to answer the purpose, provided that in addition to the spectroscopic arrangement a pair of Nicol's prisms can be attached, one below the stage and the other over the eye-piece. Both should be capable of being rotated, and it tends much to facility of working as well as to exactness of result that both the polarizing and the analyzing prism should carry graduated heads, so that their axes may readily be turned to any required degree of inclination to one another.

The instrument I employed was a large Smith and Beck. The spectroscopic arrangement consists of an adjustable slit attached to the under-part of the sub-stage below the achromatic condenser, and a set of direct-vision prisms which are inserted in the body of the instrument immediately above the object-glass.

By proper focussing, an image of the slit is thus formed by the achromatic condenser in the focus of the object-glass, and a fine spectrum obtained filling the whole field.

EXPLANATION OF PLATE XCV.

Fig. 1.—Illustrating the dividing and re-union of the bands, as described.

„ 2.—Spectra formed by ordinary and extraordinary ray, partly overlapping. The blue of the upper spectrum seen through the black band belonging to the lower spectrum. The yellow of the under-spectrum seen through the black band of the upper.

„ 3.—Rings and brushes of crystal of sugar-candy.

„ 4.—Showing displacement of the rings and absence of brushes of same crystal when light circularly polarized before and after its passage through the crystal.

„ 5.—Lemniscates and brushes of crystal of nitrate of potash.

This arrangement, it will be seen, differs considerably from the spectrum microscope in common use in which the dispersion prisms are placed close to the observer's eye, the slit being in the focus of the eye lens. The former arrangement has this manifest advantage, that owing to the distance of the prisms from the eye, the spectrum fills the whole field; also, that the apparent breadth of the spectrum can be varied at pleasure by a change of the magnifying power employed. Each form of arrangement has, however, its advantages as well as disadvantages, which it would be out of place to discuss here.

The polarizing part of my apparatus consists of two Nicol's prisms, for one of which, when desired, a double-image prism can be substituted.

The polarizing prism is carried on the sub-stage. It is inserted just above the slit in a short tube in which it can be freely turned by a graduated head. The analyzing prism is placed in the usual way—in a cap over the eye-piece.

The film of selenite to be examined, having first been mounted in balsam between two thin glasses, is placed on the stage of the microscope like an ordinary object.

It is a great convenience in this class of experiments to have the stage of the microscope not only capable of rotation in the optical axis of the instrument, but graduated.

By this means we can at any time, without displacing the film under examination, adjust its neutral axes at any required angle to the plane of polarization.

With regard to the mounting of the selenite films for examination the following method will be found convenient:—Make in the turning lathe several wooden disks about two inches diameter and one-eighth of an inch thick. Through the centre of each a hole must then be bored of about half an inch diameter. A small portion round the hole is then scooped out so as to form a cup, and in this the selenite is placed and secured with sealing-wax.

The axes of the selenites are then determined and marked on the rims of the disks.* In this way any two or more selenites can be used in combination with their axes set at any required angle to one another.

It remains only to trace the course of a beam of light in passing through the foregoing combination. First the ray, having been reflected from the mirror, passes through the slit. It is then polarized by the first Nicol's prism, after which it passes through the lenses of the achromatic condenser, and appears as an image of the slit in the focus of the object-glass. Having passed through the selenite and the object-glass, the ray enters the dispersion prisms and is drawn out into a spectrum. This is magnified by the eye-

* The graduated rotatory stage above mentioned, and which is supplied by Smith and Beck, affords a ready means of doing this.

piece through which the ray, having passed, is lastly analyzed by the second Nicol's prism.

The loss of light from the number of the above media is not so great as might be supposed, still an intense source of light is desirable for satisfactory results. A good artificial light placed close to the mirror will be found the best. In diffused daylight rays are apt to enter the object-glass by reflexion from the brasswork without first passing through the polarizer, by which the beauty of the spectrum is impaired.

To understand the bearing of the experiments, it is necessary to keep in view the different effects of a doubly-refracting film upon polarized light, according to the position of its axes, with respect to the planes of polarization.

Suppose we take a film of selenite, such as those commonly sold as an adjunct to the polarizing microscope, giving, as its two colours, a pinkish red and its complementary green. Such a film will, if examined between two Nicol's prisms, act on the light according to the following laws:—

1st. When a neutral axis of the film is in the plane of primitive polarization, the film will exercise no influence on the light; if therefore the prisms are set with their axes perpendicular the field will remain dark, if the prisms have their axes parallel the field will contain only white light.

2nd. If the prisms are placed with their axes perpendicular, and the film is made to rotate, there will be four points of darkness at each quarter of a revolution, *viz.* when an axis of the film is in the plane of polarization, and between these four points, the same colour (say green) will occur.

3rd. If the prisms are set with their axes parallel, and the selenite is rotated, the field will be white at the four points where it was previously dark, and of the complementary colour (red) between each of these four points.

4th. If the selenite is fixed with its neutral axis inclined 45° to the plane of primitive polarization, and the analyzer made to rotate, the field will be alternately red and green in the four quadrants.

5th. The colours are always of maximum brightness when the axes of the prisms are perpendicular or parallel, and the axes of the selenite inclined 45° to the plane of polarization.

Suppose, now, we repeat the above experiments, using the polarizing spectrum microscope above described, and let us call the point in the revolution of the selenite at which either of its axes is in the plane of primitive polarization, the zero point, from which the number of degrees through which it is turned are measured.

Let the prisms be set with their axes perpendicular to one another, and the selenite rotated on the stage. The spectrum will

of course vanish at the four zero points. Between these points, however, remarkable phenomena occur. A person unacquainted with the true nature of the colours of polarization, and proceeding on the analogy of homogeneous light, might expect to get a spectrum consisting only of green rays, seeing that that is the colour of the field when the spectrum arrangement is removed. This however is not the case, and the result very beautifully illustrates to the eye what is well known theoretically to be the true nature of these colours. What we obtain is a continuous spectrum consisting of all the prismatic colours, in greater or less intensity, with the striking peculiarity that there is a well-marked dark band in the red, similar in appearance to the well-known absorption-bands which many substances produce in the spectrum, only blacker and better defined than these are ever seen.

The following is the mode in which the band makes its appearance. As the zero point is passed, the light first makes its appearance in the green of the spectrum, from which point, as the selenite is rotated, the light opens out in both directions. When the light reaches the red, the black band makes its appearance, and attains a maximum blackness when the selenite is at 45°, *viz.* when, without the use of the dispersion prisms, the field would contain green light of maximum brightness. When this point of revolution is passed, the band again fades, the spectrum becomes obscured at each end, the darkness creeping in towards the green, till at 90° the spectrum has again vanished. The same phenomena recur at each quarter of a revolution.

Let the Nicol's prisms now be set with their axes parallel, and the same selenite rotated on the stage as before. The result is what we should be led to expect from the last experiment. At the zero points the selenite exercises no influence, and we have a continuous ordinary spectrum. As a zero point is passed, however, a dark band makes its appearance, but this time in the green rays. The band is at first faint and nebulous, but becomes blacker and sharper as the stage is rotated, till at 45° it attains its maximum. The spectrum in this experiment never vanishes, but is apparently quite continuous throughout, save for the appearance of the black band.

Lastly, let the selenite be fixed at 45° from the zero point, and the analyzer rotated. We have now a combination of the two previous experiments. The band in the red appears alternately with the band in the green at each quarter revolution, the former being at its maximum when the axes of the prisms are perpendicular, the latter when these axes are parallel.*

* If the axis of the selenite makes a greater or less angle than 45° with the plane of polarization, the result is that while the same band still recurs after 180° of a revolution of the analyzer, the complementary band is no longer separated from it by 90°, but by a greater or less angle.

The above are the appearances which present themselves in the case of most films of selenite of a medium thickness. In some cases however, two, or even three, black bands occur simultaneously, these being always followed by as many complementary bands, when the analyzer is turned through 90°. The number of bands can generally be multiplied by using two or more films in combination, and the appearances can be still further varied by changing the degree of inclination of the axes of the two films to one another. If the two films are placed with their similar axes coincident, we obtain of course the spectrum appropriate to a film equal to the sum of the thicknesses of the two films, while, if dissimilar axes are superposed, the spectrum is that due to the difference of the same. I have two films which, when properly combined, give no less than six well-marked bands simultaneously.

But the most striking of the phenomena presented by films which give more than a single band, remains still to be noticed, *viz.* the motion of the bands along the length of the spectrum. This can generally be easily seen by using two films in combination, and properly adjusting their axes.

The following may be taken as an illustration of this experiment, of which many varieties may be made. (Fig. 1, Plate XCV.) Suppose the two films are so adjusted as to give two black bands, one in the orange and one in the blue, which we may call *a* and *b* respectively. On rotating the analyzer, each band is seen to divide into two halves. The right-hand half of band *a* runs along the spectrum, and unites with the left-hand half of band *b*, which advances to meet it, the two coalescing into a single band in the green. At the same time that this has been going on, two entirely new bands have made their appearance. These seem to originate respectively beyond the visible rays at each end of the spectrum, and to advance in opposite directions till they are met respectively by the left-hand half of the original band *a* and the right-hand half of the original band *b*. The result is, that when the analyzer has been turned through 90°, we have a spectrum with three black bands, one in the extreme red, one in the green, and one in the indigo.

Continuing still further to turn the analyzer the above phenomena are reversed. Each of the three bands split into two, moving in the reverse of their former directions, until when 180° is reached the original spectrum with its two bands recurs.

A curious variety of this experiment occurs when a circularly polarizing film is interposed between the analyzer and the film producing the bands. The nature of the movements of the bands is now entirely changed, the order of motion being all in the same direction, and the bands appearing to chase one another along the length of the spectrum, making their appearance at one end and

disappearing at the other. To produce this effect, the "band-producing" film should be set with its neutral axis at 45°, and the circularly polarizing film superposed with its neutral axis in the plane of primitive polarization. If the axis of either film is turned through 90°, the motion of the bands is reversed; *i.e.* if the bands formerly moved from left to right, they now move from right to left. If the two films are both placed with their axes at 45° to the plane of polarization, the only effect of the circularly polarizing film is to alter the position of all the bands by a corresponding amount (*i.e.* to increase or diminish their refrangibility), without affecting the nature of their motions.*

A very pleasing and beautiful variety of the foregoing experiments may be obtained by using a double-image prism as the analyzer instead of the Nicol's prism. Two spectra formed respectively by the ordinary and extraordinary ray are thus obtained, which by rotating the double-image prism may be made to lie parallel to one another, or be partially superposed at pleasure, while by turning the polarizing prism the spectra can be made of any desired relative intensity. Suppose that we adjust the two prisms with their axes at right angles, and interpose the selenite used in the first experiment, which gave a band alternately in the red and in the green, we get now two spectra parallel to one another, the band in the red of the one occurring simultaneously with the band in the green of the other. The two bands are thus seen to be strictly complementary, for the band in the red of the one spectrum appears, attains its maximum, and vanishes simultaneously with the similar changes of the band in the green of the other spectrum. This coincident appearance of the bands, moreover, is independent of the inclination of the axis of the selenite to the plane of polarization, the only effect of a change in which is to increase or diminish the maximum intensity of both bands alike, a result which, as has been noticed, does not hold with regard to the alternation of the two bands in the same spectrum.† (Fig. 2, Plate XCV.)

When the two prisms are placed with their axes parallel, so that the two images of the slit are seen alongside one another, and consequently the two spectra partially superposed and different colours mixed, the appearance of the bands is very striking. A band occurring in either spectrum is no longer black, but of the colour of that part of the other spectrum which coincides with it. The appearance is, in fact, as if a stripe had been cut out of the one spectrum through which the colour of the other spectrum is seen, while on either side of the band we have in striking contrast

* The effect of circularly polarizing the light before it passes through the selenite, is simply that the occurrence of the bands is irrespective of the inclination of the axis of the selenite to the plane of primitive polarization, and depends solely on the position of the analyzer.

† See note on p. 138.

the colours due to the compounding of the different parts of the two spectra.

The beauty of the effect depends of course greatly on the extent to which the double-image prism separates the two images. It should be so cut that the compound colours caused by the overlapping of the spectra shall be as different as possible from either of their constituent colours. The selenite should then be set at 45°, so as to make the spectra of equal and the bands of maximum intensity.

With films which give numerous bands the effect is very beautiful, and may be still further enhanced by rotating the polarizer, when the bands will shift their position, at the same time changing their colours.

Experiments with Sections of Double-refracting Crystals giving Coloured Rings.

The coloured rings produced when polarized light is transmitted through a double-refracting crystal cut perpendicularly to its axis, have always been admitted to be among the most beautiful of the phenomena which the science of optics can produce.

When homogeneous light is used it is well known that the rings assume entirely the colour of the light used, the spaces between the coloured rings being black.

The splendour of the phenomena, however, obtained by the use either of ordinary or of homogeneous light, is incomparably inferior to that displayed by projecting the rings against the spectrum. The spectrum microscope is admirably suited for this exhibition.

The method I adopted was simply to place the section of the crystal immediately over the eye lens of the microscope, and between it and the analyzing prism.

The rings are thus seen of every colour in the spectrum, alternating with jet-black rings between each, those in the red being the broadest, and the breadth of the rings gradually diminishing to the most refrangible end of the spectrum.

It is impossible to give any satisfactory idea of the appearances by mere description, and no little skill or labour would be required to paint any adequate representation of the effects seen in some of the following combinations.

Take, as an example, a section of a crystal of sugar which gives a very fine system of rings. (Fig. 3, Plate XCV.) I have counted easily as many as forty-five when projected against the spectrum. This crystal is one of those which gives in polarized light two black brushes, not a black cross like Iceland spar. When the Nicol's prisms are at right angles the brushes are at their

maximum intensity, and the spectrum with its series of rings is seen to be cut in two by the jet-black brushes. When the analyzer is turned through 90° the brushes which would now, if seen by ordinary polarized light, be white, are of every colour in the spectrum according to the part of it they fall upon, and shaded off at their sides by a nebulous haze of colour through which the black rings are visible.

In intermediate positions of the analyzer the brushes become entirely nebulous, so that the rings can be seen through their whole extent. In this position of matters the circle appears divided into four quadrants, and the rings are distinctly seen to be dislocated so to speak, *i. e.* the rings in the alternate quadrants are pushed out so that each coloured ring in the one quadrant is continuous with a black ring in the next.

This effect is still better seen by circularly polarizing the light before its passage through the crystal. (Fig. 4, Plate XCV.) The effect of this is a curious one. Instead of the circle being divided into four alternate quadrants, it is now divided into two semicircles, the rings in the one being alternate with those in the other. The semicircles are separated by two narrow coloured brushes which revolve with the analyzer, and seem as if they swept out the black rings in the one segment to be replaced by the coloured rings of the next. If we again circularly polarize the light by interposing a second circularly polarizing plate between the crystal and the analyzer, the brushes entirely disappear, and both the black and the coloured rings are continuous throughout, forming perfect circles.

When the analyzer is rotated through 90°, the centre of the system which was formerly black is now coloured, and, at the same time, all the black rings have exchanged places with the coloured rings, the change being effected by a lateral displacement in opposite directions of the two halves of the circle.

If for the second circularly polarizing film we substitute a film of a different thickness, the rings assume curiously distorted forms. With one film which I used the rings became ellipses, with another they all united so as to form a circular helix, which appeared to unwind like a screw as the analyzer was turned.

The appearances produced by using different crystals are of course similar, *mutatis mutandis*.

By circularly polarizing the light before and after its passage through a crystal of nitre (Fig. 5, Plate XCV.), the brushes are wiped out, and the lemniscates are beautifully seen, unbroken throughout.

When a crystal of Iceland spar is used, and the Nicol's prisms set with their axes inclined 45° we get eight segments, of which the four light segments look as if they stood out in relief against

the dark segments, while the sections of the black rings, especially near the centre of the system, look more like straight lines than circular arcs, and form a system of octagons.

The effect upon the rings, produced by placing on the stage a film of selenite in the position in which it should give the black bands previously described, is a strange one. Instead of a black band occurring, the coloured ring belonging to that part of the spectrum is seen to split into two. It sends off a branch as it were from its lower part, which shoots across the adjoining black ring, and joins itself with the lower part of the next coloured ring. This last ring then in turn sends off a branch from its middle part, which in like manner unites with a third ring, which in turn does the same to its neighbour. All this takes place within the space which should be occupied by the black band.

The beauty of these last experiments, wonderful as it is, may be still further enhanced by the use of a double-image prism as the analyzer. The result is analogous to that obtained with the same arrangement in the case of the selenite previously described. We now get not only two spectra, but two systems of rings, which, by superposing the spectra, may be made to interlace with one another. Wherever a black ring of the one spectrum crosses a black ring of the other, the intersection is of course still black. Where a coloured ring of the one system crosses a black ring of the other, it retains its original colour; but if a coloured ring crosses a coloured ring, the intersection is of the resultant colour of the two combined.

Still more complex figures are got by employing two or more crystals in combination.

Indeed, there is no end to the variety of exquisite beauty, both in colour and in pattern, which a little ingenuity may produce. Pigments would be almost as helpless as words in representing many of these. The appearance produced by a single crystal with a double-image prism as analyzer, may be not inaptly compared to a tesselated pavement of every colour made for a fairy palace, while that produced by combining two crystals may be said to resemble a suit of chain armour wrought for a fairy king, in jewels of which no two are of the same hue.—*Read before the Royal Society of Edinburgh.*

IV.—*Remarks on some Parasites found on the Head of a Bat.*

By R. L. Maddox, M.D.

Plate XCVI.

In the early part of the month of April was received from my friend Dr. P. Davidson, Staff Surgeon, late of Royal Victoria Hospital, Netley, the head of one of those strange creatures which have furnished, in times long past, the pattern for the aëronautical addenda to man's mythical image of the "Evil one." See a plate in W. Young Otley's fine series, entitled 'The Italian School of Design,' published 1823, of part of a fresco by Giunta Pisano, of the thirteenth century, in the church of St. Francisco at Assisi, on the high road between Perugia and Foligno, dedicated to St. Francis, the founder of the order of monks that pass by his name, where the sorcerer, Simon Magus, is represented as being borne away by no less than seven of these imps; possibly his beautiful concubine Helena travelled by a similar mode of transport, though on this we are not enlightened. The wings, when compared to like appendages furnished to the saints, are noticed as very different in structure, as seen in the colossal figures by Cimabue in the same church, and copied by Mr. Otley in his valuable work. Now, it is curious to notice, the artist in his design has given to the expansion of the interfemoral membrane eight metacarpal and digital prolongations, whereas in the usual figures of the bat in works on natural history we find only five.

This "unclean animal" of the Hebrews—Attaleph—of the mammalian division, was placed in the forbidden list amongst birds: from this, and being so active in the period of darkness, so anomalous in the general appearance, so destructive in some countries, and so strange in their habits and haunts, we may possibly find some reason why this form of wing should have been engrafted on the

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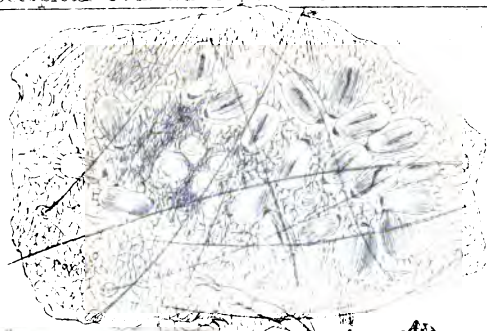
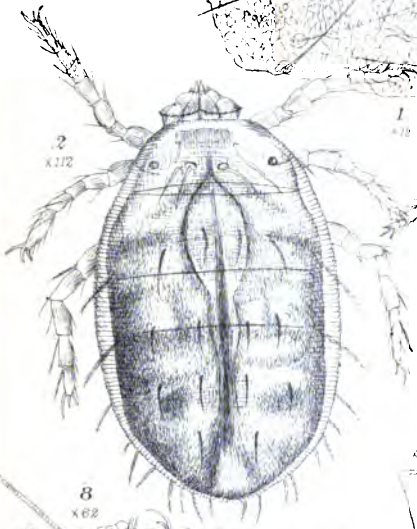
DESCRIPTION OF PLATE XCVI.

Fig. 1.—A group of parasites, and the openings made by those detached.
„ 2 and 3.—The dorsal and ventral aspect of two of the insects slightly compressed, after remaining in glycerine and sweet spirit of nitre.
„ 4.—The ventral aperture surrounded by wavy lines in the skin; there were four hairs near, but these were not figured.
„ 5.—A torn part of the skin showing the wavy lines, also the inner surface and some muscular (?) trabeculæ.
„ 6.—The third leg with the plumose hairs and the trifid claw.
„ 7.—The buccal apparatus more highly magnified.
„ 8.—Two of the openings in the dermis where the insects had been attached, showing the thickening resulting from the irritation, and a well-marked bundle of nerves, * *.
„ 9.—The single minute insect found with Figs. 2 and 3, but free and amongst the hairs (? immature male).
„ 10, 11.—The two inferior maxillæ. Fig. 10, side view of the left one; Fig. 11, view of the right one as seen from above, in the natural closed position when the insect is forcibly removed from the skin.

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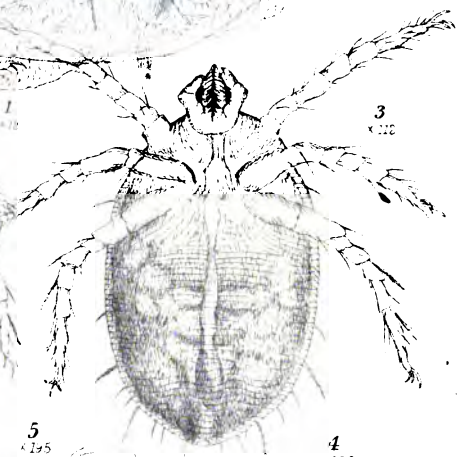
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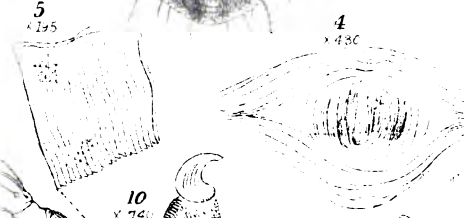
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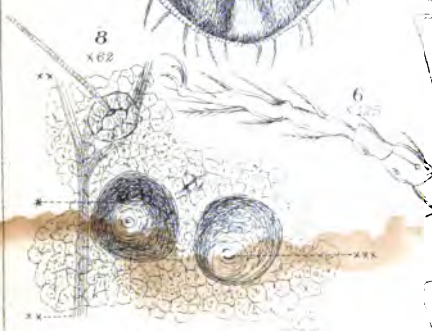
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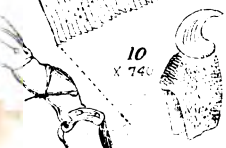
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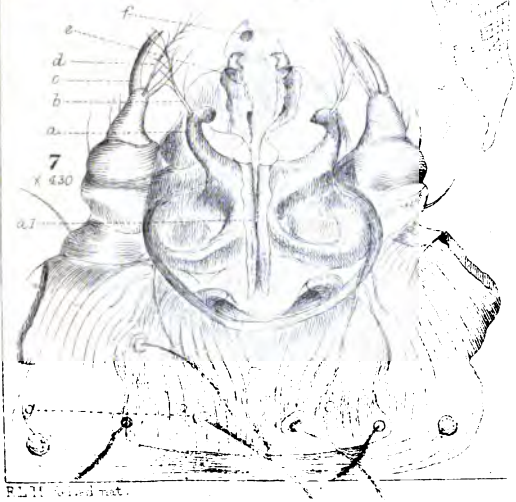
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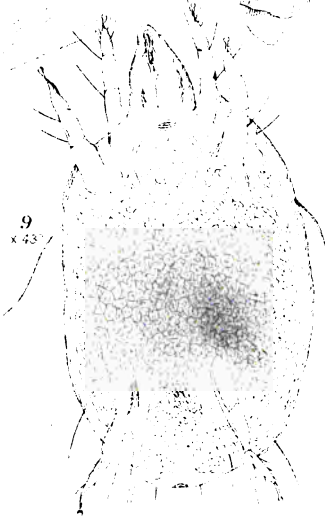
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figure of Satan. The head sent belonged, I believe, to *Vespertillio pipistrellus*, of the European species of Vespertillionidæ, a series of creatures that even still, in the minds of the vulgar, continue to inspire a singular dread, from superstitious feelings about the blood-sucking propensities of the vampire. Though fruit and insect feeders, the latter do not object in confinement to the dainties of raw meat. Examining this head carefully, which had travelled from Scotland, numerous small insects were found attached to the basal portion of the ears internally, and to one of the earlets. Two or three of these little parasites showed signs of life when touched; they were of a light fuscous colour, with dark irregular patches, and one darker than the rest, that passed down the body in a somewhat wavy manner; they were grouped at the feast in batches, almost mouth to mouth, one batch containing upwards of twenty (*vide* Fig. 1), others numbering fewer, and one only five.

Unfortunately, being at the time pre-occupied, it became a serious question how best to preserve the head for future observation. The ears and earlets were cut off at the base, and divided near those parts showing the presence of the parasites; the head and other parts of the ears were placed in Verril's solution, A, and the portions with the insects in two small bottles, one with glycerine and sweet spirit of nitre, equal parts; the other, in rectified spirit and acetic acid, equal parts, and set aside. They were not looked at until the end of July when the specimens were found in each in good condition. For closer examination several of the parasites were detached from the skin by means of force, and the deposit in the liquid employed, carefully observed for any that might have detached themselves; but only two were thus found. The insects, seeing they were all hexapods, were at first taken for some of the Ixodidæ, the "degraded diptera," as Dr. Duncan calls them, in his 'Transformations of Insects,' that form the "connecting link between the true insect, the spider, and the mite." A nearer study of them has led to the following remarks, which I feel some hesitation in offering to the readers of the Journal of the Society, seeing I cannot find a satisfactory clue to the different stages of life, nor indeed, in any of the books at my command, the correct name of the insect or rather insects, for there are two figured in the Plate. The average size of those in the group may be fixed at the 58th of an inch. The body is thick, somewhat oblong in shape, truncated at the anterior part, divided by three fine transverse lines or divisions, and presents a few rows of stiff plumose hairs, set on the back, sides, and posteriorly: it has the appearance of being filled with dense irregularly granular and fatty matter, besides the dark band of recent nutriment before alluded to. When pressed, the body is found to be rugose, and covered with wavy lines as in the palm of the hand, and beautifully seen in many of the Acaridæ. No eyes were seen that could

be satisfactorily claimed as such. The head is very much depressed —if head it can be called—is somewhat of a pentagonal figure, the two stout cheliceræ forming four of the sides (the apex projects as a double point), the base or remaining side being made by the attachment to the body (*vide* Figs. 1, 2). The "instrumenta cibaria" (Fig. 7) are placed within a sort of neck, like the top of a tied sack; the labrum appears to consist of a thin chitinous plate united posteriorly to the cervico-thoracic ring, and in the middle on the under surface, to two long stylous processes, *a* 1, connected with the maxillæ *a*, situated either side of the median line, and laterally to the basal joint of the cheliceræ. It overlies a complicated framework that forms or supports the maxillæ, and that branches backwards by two curved processes, to be apparently hinged to two incurved processes arising from the thoracic attachment. The middle portion of this framework looks to be enfolded internally, and to be connected to two stout inwardly-curved parts, *a*, strongly tipped with chitine at the ends, and supporting on the anterior and outer portions a feathery bristle—which pieces taken together may probably be said to form the labrum, superior maxillæ, and maxillary palpi, *a* 1, *a*, *b*. Beneath are two portions, *e*, forming the basal joints that support each a stout curved tooth, slightly indented (Figs. 10, 11). In the natural position they are placed side to side; but when in action cut or tear horizontally, and represent the inferior maxillæ: applied together and with the inner portions of the upper maxillæ, they form a more or less perfect suctorial tubular cavity; situated laterally and strongly attached to the cephalic framework at their bases are two stout conical cheliceræ, four jointed, and supporting a long bifid tooth or claw, *c*, having at its base on the inner side a compound feathery brush, *d*. The inferior lip seems to consist of a thinnish chitinous membrane that unites these parts inferiorily and laterally, to which is attached what appears to be a saccular eversion of the ligua or pharynx under compression, as in the example from which the figure was taken, almost the whole of the granular contents of the abdominal cavity were ejected through the mouth from the pressure. This ligua (?) has two conical processes (? glandular) and a circular opening, *f*. At the cervico-thoracic junction on the upper surface, behind, is a thin chitinous plate with a waved or curved outline, supporting three fine hairs on the front and four near the back edge, the two lower central ones being much larger than the others, (?) ocellar at the base; though more laterally on the same line are seen two somewhat opaquish oval projections, which may be visual organs, and beyond these on the same sweep of curvature, two equal if not a trifle larger areas than the central ones are visible, which are suspected to be tracheal orifices. The laxity of the tissues after the use of liquor potassæ, and the great displacement that occurs in

neighbouring parts from slight compression, almost prevent the possibility of assigning to each the proper position; while in the natural state, being often covered with exudation from the wound and filled with dense grumous matter, exactitude in the description is by no means easy. At Fig. 3 is given the ventral aspect of another insect and the mode of attachment of the legs: one, the hind leg, is shown at Fig. 6; it is furnished with numerous stiff and branched hairs; the tarsus, which is longer than in the other legs, terminates, as each one does, in three claws, the outer two uncinæ being expanded, stronger, and more curved than the middle or long one. The ventral aperture is shown in Fig. 4. In Fig. 5 is depicted a portion of the dermic tissue of the saccular body, duplicated at one part, and also showing the inner surface, to which can be seen attached five (?) muscular bands,* stretching between the surfaces. No transverse striæ were seen on these bands, but they are supposed to be muscular in action.

This description differs in several particulars from the small hexapod mite, 1 mm. in length, the *Argas pipistrellæ*, found attached to the body of the bat, and described by Lucas in his 'Cours Complet d'Histoire Naturelle,' tome xiii., p. 483, but borrowed from M. Audouin's observations, the tarsi terminating in two small hooks, &c. Not finding any satisfactory description of this insect I felt much puzzled whether these mites should be considered as belonging to the "degraded diptera," or whether they were merely the larval form of some psoreptes, or itch mite. Accordingly, considerable time was spent in looking for more advanced specimens, as the whole of those grouped together appeared to belong to only one sex, and were possibly the early stage of some well-known form. After much patience I was rewarded by finding among the hairs, one, and only one specimen, very much smaller, differing greatly in appearance from the rest, and approaching more nearly the character of the male itch insect. Yet on higher magnifying, it was seen to differ considerably (*vide* Fig. 9). Its size is about 165th of an inch. The dermic tissue is plain, or shows no transverse lines. Six of the legs are provided with a small disk; the other two of the hind legs (for in all they are eight in number) consist apparently of two long joints, one being setigerous; their position, distant from the anterior pairs, is similar to the itch mite—possibly an immature male—though in the male itch insects I have examined, if I remember correctly, the outer hind legs are provided with bristles, the inner with disks, which is the reverse in the insect under consideration. Looking at these points, and not knowing whether the single insect had any relation to the rest, under present circumstances it will, it is thought, be better to leave the matter open, as to their position in the family of Arachnidæ.

It must not be lost sight of, that amongst some fifty insects found on the two ears, we can scarcely suppose some of them should not be mature, if they belonged to the family of Acarea. This view led me to seek carefully for some ova beneath the skin, or the shells attached to the hairs; yet nothing was found beyond portions of the inflammatory exudation carried up by the young hairs, or adherent where the hairs had touched the irritated surfaces; nor was any trace of an ovum found in the bodies of those cut open or crushed under the dissecting microscope; though in the mass of granular matter, four small nuclear-looking and granular masses, larger and more firm than the rest of the large granules, were found, two on opposite sides of the lower half of the abdomen, but what they were it was impossible to say correctly. If the foregoing view be correct, amongst some of the ova we may expect the male to be reproduced. Again, we have no evidence they were not, in the full sense of the word, adventitious—just passing one part of their lives in a luxurious feast; yet, if they belong to the Ixodeæ, may we not suppose either that one or more females attached themselves to the bat, and then gave birth to the colonies, for they are described as depositing or producing by a continuous pont of many days, upwards of a thousand glutinous eggs (?). Yet the gestative state of the Ixodes is said to be continued by the insect detaching itself and falling to the ground for completion; so this hardly admits of its performance on the bat.

The singularly peculiar, if not almost unique character of the generative act of the Ixodidæ, described by Professor Gené, of Milan, and translated in abstract by Mr. A. Tulk, in the 'Annals and Magazine of Natural History,' vol. xviii., to which I beg reference for those interested in the history of these minute creatures, and which will amply repay a perusal, I can only in part quote here. In brief it may be stated, the male inserts its rostrum into the orifice, situated upon the middle of the sternum, between the coxæ of the last pair of legs. Mr. Tulk points out the "very striking relation, if only approximative in kind, between the organ employed by the male Ixodes to copulate with the female, and the palpi as ministering to similar uses in the Araneides, or true spiders." That the female afterwards depresses upon the sternum all the palpi that compose the rostrum, when there is seen to be "protruded from the duo-cephalic plate, a turgid vesicle," terminated by two lobes, "vesica biloba," having at the apex a most minute aperture. "When this organ has been well dilated, so as to pass beyond the rostral palpi, the animal erects the pectoral canal, and gives exit to the oviduct," and "proceeds at once to disemburden itself, between the lobes of the vesica. This clasps, compresses, and appears as if sucking the oviduct for a few seconds; but often the oviduct is retracted, and re-enters the sternum, leaving an egg

between the lobes of the vesicle, which clasps it firmly, turning it to and fro in all directions, and vibrating now and then in a spasmodic manner. Four or five minutes having elapsed, during which time the ovum remains between its lobes, the vesicle disappears by re-entering its internal situation. The ovum is left upon the inferior labrum, and this being elevated, along with all the palpi that compose the rostrum, thrusts the ovum upon the duo-cephalic plate, or in front of the body—these acts being renewed for as many ova as the female may have to discharge." A series of very interesting experiments made by Professor Gené are then related, bearing upon the correctness of the foregoing.

Before drawing this lengthy article to a conclusion, it is necessary not to omit noticing that the parasites of the bat's ear prefer the inner surface, where the hairs are fewest and the glands most numerous. They collect in companies (*vide* Fig. 1, where twenty are seen, and seven apertures made through the dermis left by the detached insects). This figure represents a very hungry lot, and they generally, so far as could be judged, seek those parts of the inner surface of the ear that are well provided with nerves; a nerve bundle is shown at **. Moreover, they appear to prefer to use the aperture through which the hair protrudes, than to be at the trouble of tearing one open. They seem to fix themselves much in the same way as the tick to one spot, and by their presence cause a considerable amount of mischief, inducing much congestion and thickening of the tissues beneath. Two such apertures are represented more highly magnified at Fig. 8, one having a minute hole at *; the other, ***, showed no such opening. The hair follicle and sebaceous glands appear totally destroyed in most of the openings; the cartilaginous tissue seemed to suffer only slightly; the vessels looked enlarged; a large nerve bundle is seen at **, **. In these examinations the skin was dissected off both sides of the cartilage, to obtain the necessary transparency; portions were subjected to various reagents, and showed that the ear of the bat seems almost in its amount of nerves, &c., to rival its wing, described in abstract in the June number of the Society's Journal, p. 272, from Dr. Joseph Schöbl's most interesting observations. However, it would be only in recent and injected specimens any attempt could be satisfactorily made in the examination of these organs. The incompleteness of this paper is regretted, but it is hoped sufficient has been stated to induce other observers, when opportunity offers, to give us the benefits of a more extended examination.

V.—*Note on the Resolution of Amphipleura pellucida by A. Tolles's Immersion $\frac{1}{5}$th.*

By Assistant-Surgeon J. J. Woodward, U. S. Army.

In my paper on the use of the Nobert's plate, written in April last, and published in the July number of this Journal, I found myself compelled to make a few remarks on the objectives of Mr. Tolles, of Boston. While complimenting this maker on his excellent workmanship, I felt constrained to say that I had not found that his objectives excelled those of like powers by other first-class makers.

Late in June of the present year, however, I received from Mr. Tolles a $\frac{1}{5}$th, the performance of which is so remarkable that I take pleasure in drawing attention to it.

This objective is so made as to work either dry or immersion, and it is of its performance when used wet that I desire to speak. Its magnifying power at 48 inches distance between micrometer and screen (without an eye-piece) is 250 diameters when corrected for immersion uncovered, 275 diameters when corrected for the thickest cover through which it will work. It is therefore of rather higher power than a $\frac{1}{5}$th, but less than a $\frac{1}{6}$th.

Now, with this objective I find no difficulty in resolving *Amphipleura pellucida*, the objective successfully displaying the transverse striæ on all but the most minute and difficult frustules.

To illustrate the character of the performance, I send you herewith two positives on glass from negatives taken by this $\frac{1}{5}$th.*

The first shows two frustules magnified 256 diameters. It is of course necessary to use a lens, or a low power of the compound microscope, to see the striæ, which will be found to be quite sharp.

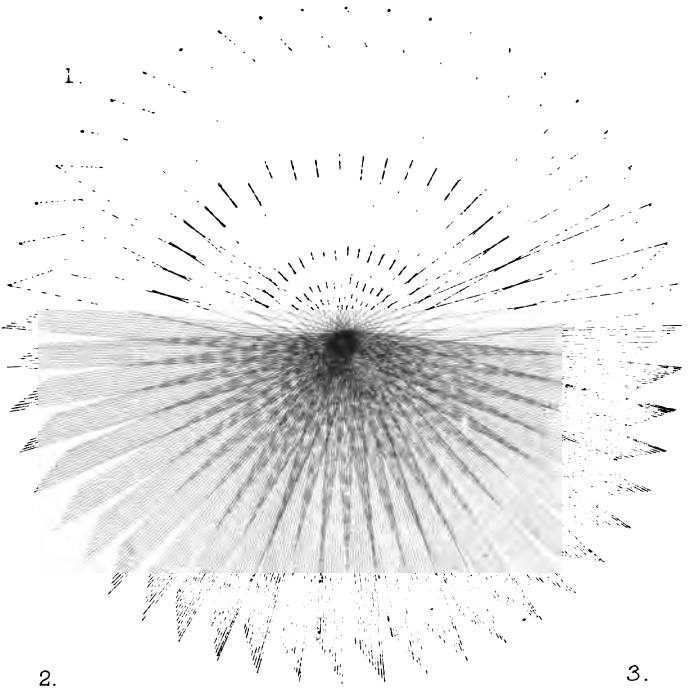
The second shows the same two frustules magnified 920 diameters. The striæ can be seen with the naked eye, still better with a lens.

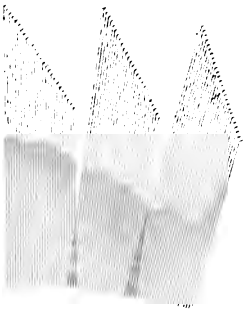
I send no paper prints of these negatives, because, on account of the fineness of the striæ, as seen with the above powers, they would not be satisfactory.

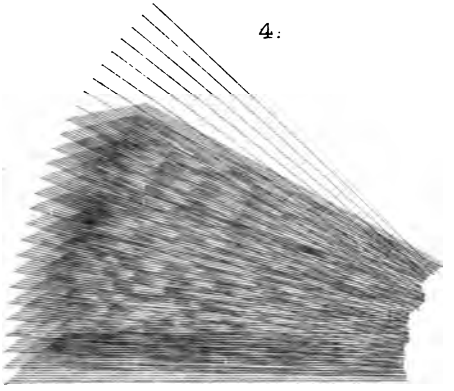
I add, however, a third positive, representing the same frustules magnified 1140 diameters by the immersion $\frac{1}{16}$th of Powell and Lealand.

This picture certainly shows that the new $\frac{1}{5}$th cannot be claimed to supersede the highest powers at present in use, yet, nevertheless is not, in my opinion, injurious to the $\frac{1}{5}$th, for it must be mentioned that the immersion $\frac{1}{16}$th of Powell and Lealand, with which this picture was taken, magnifies at 48 inches distance, without an eye-

* The photographs are admirable. They are at the Society's rooms, where they may be seen.—Ed. 'M. M. J.'







W. West & Co. imp.

Optical Appearances of Stars ruled on Glass & Steel,

piece, 900 diameters when used *un*covered, and 1100 when used covered; and it is certainly highly creditable to an objective of so much lower power to be capable of resolving so difficult a test as the Amphipleura.

From this performance I expected that the new $\frac{1}{5}$th would at least resolve the sixteenth band of the Nobert's plate; but in this I have hitherto been unsuccessful, getting indeed handsomely through the fifteenth band with it, but no further. Now, as the objective resolved Amphipleura frustules with striæ as fine as 96 to the $\frac{1}{100}$th of an inch, I can only account for this circumstance by supposing a greater difficulty in the case of striæ of equal fineness on the plate, as compared with Amphipleura. This circumstance, which had previously escaped my notice, I have since confirmed by comparison with a number of different objectives.

In conclusion, I beg you to show the photograph accompanying this note to any of your readers who may take an interest in the performance of objectives of moderate power. I should be very happy to hear from them how this result compares with the work of the best modern English $\frac{1}{5}$ths, particularly as I have access at present to no English glass of this power constructed within the last two years.

VI.—*Micro-ruling on Glass and Steel.* By John F. Stanistreet, F.R.A.S. With Illustrative Remarks by Henry J. Slack, F.G.S., Sec. R.M.S.

Plate XCVII.

By the kindness of John F. Stanistreet, Esq., F.R.A.S., of Liverpool, I have lately been able to examine some very beautiful specimens of ruling on glass, and also on steel, executed by him with a machine of his own contrivance and making, of which some particulars are subjoined.

The first specimen which I received was a star exquisitely ruled on a small circle of covering glass ($\frac{4}{10}$" diameter), and it cannot be better described than in Mr. Stanistreet's own words:—"The mounted disk, which I herewith enclose, has a star of 50 rays, or bands of lines, placed radially, each band consisting of 26 lines ruled parallel and equidistant, the $\frac{1}{2000}$th of an inch apart. The star therefore contains 1300 of such lines. The lines are purposely scored very strongly to increase the intensity of the diffraction spectra, as I find that lines very much finer and closer (which are just as easy to rule up to about 10,000 to an inch) are not so effective for this purpose."

Some beautiful diffraction spectra can be obtained with this star,

but to the microscopist it is instructive from the various perspective appearances it presents. From the principle on which it is ruled the lines composing each ray proceed for some distance before coming in contact with the lines belonging to adjacent rays, as shown in Fig. 1, Pl. XCVII. Each ray, at its peripheral end, commences with what looks like a sharply-cut dot, the first impact of the diamond point with the glass, and the lines composing each ray gradually diminish in length, giving the wedge-shape shown in the figure. When the star is viewed with a low power (say 3″), with dark-ground illumination, the optical effect of the rows of bright dots, with which the lines commence, is to suggest that each ray stands up from the general plane of the glass, that all look like a number of spokes of a fan placed, more or less, vertically on the table. A higher power (⅔rds) makes this more striking, as in Fig. 2. The position of each ray, with reference to the angle at which a light strikes it, determines how much it slopes either right or left of the vertical plane, and one or two rays will appear nearly in that plane. Keeping the illuminating apparatus stationary, and revolving the stage, causes the apparent slope of each ray to vary, and if any pair of rays be selected for particular observation, they will be seen to undergo curious apparent changes of position. At one point of the stage rotation it will appear as if the eye beheld the *outside* of one ray and the *inside* of its neighbour, while at another point the appearance will be reversed, and it will look as if the outside of the first and the inside of the second had come into view. If the object is then moved so that the wedge-shaped ends of the rays are thrown out of field, the eye is somewhat tempted to consider adjacent rays as not quite in the same plane, but the striking illusion just described has disappeared.

After proceeding separately for some distance, the rays commence their contact, and the intersections of the several lines composing them produce a secondary star, with finely-pointed rays, gradually broadening towards their bases. It is easy to illuminate these secondary rays so as to make this star appear in a plane higher than that of the primary star, and to give an appearance of solidity to each ray.

Near the centre of the star, where the rays meet, the aspect is beautifully *watered* and the lustre is silvery, or delicately iridescent, according to the illumination. In the centre is a clear space, and this has the aspect of a deep hole, an appearance much assisted by the curvature of the lines as they come to the point.

The appearance of concentric *scorings* seen on the secondary star arises from intersections. It is striking with powers under ½ inch, but with ½ inch and upwards they grow fainter, and disappear in a conflict of cuts.

Not to enlarge further on this particular star, it will be seen

that in addition to being an object of great beauty it throws light upon causes of deceptive appearances, which may usefully warn us against errors of interpretation.

A simpler glass ruling shows the tendency of the eye to be best satisfied with such a mode of focussing and illuminating intersecting lines as gives the aspect of one set being above or below the other. When the lines are smoothly cut, there is also a tendency to prefer that mode of viewing them which gives the aspect of solid threads raised above the surface. The smoothest cuts preserve this character, more or less, with high powers; and as I have shown in a former paper, the very smooth cracks of silica films are exceedingly deceptive.

In a glass star of another description, I find bands of converging lines in alternate sets with parallel lines. There are 12 bands of 20 parallel lines each, in each of 10 rays, besides 25 radial lines. "Thus," as Mr. Stanistreet says, "the intersections are extremely numerous." The twelve bands of parallel lines prevent the appearance, described in the first star, of a number of spokes of a fan composed of numerous silver wires, and arranged more or less vertically. These fan-spokes are crossed by the radial lines, and with dark-ground illumination it is easy to show the latter as distinctly *overlying* the fan-spoke lines. It is also easy to get an opposite appearance in some parts of the same field, and to see the fan-spokes raised above the radial lines. It should be mentioned that the radial lines are far apart as compared with the close bands that form the fan-spokes. If this object were a great deal smaller, and nothing known of its real structure, the difficulty of interpreting the optical appearances would be great.

In a star composed entirely of closely radial lines, those which catch the most brilliant light appear to stand above the rest.

Having suggested to Mr. Stanistreet that beautiful and curious effects might be expected from applying his ruling apparatus and remarkable skill to steel, I soon received from him an exquisite star, much like the one first mentioned, on glass. This star is composed of "50 radial bands of 40 parallel lines in each band;" the general pattern being like Fig. 1.

Held in a bright light this star exhibits a very elegant appearance of watered silk, with delicate prismatic tints. Under the microscope, with 3 or 4 inch power and illumination with a silver reflector, the star appears as if suspended in a dark atmosphere, or in a bright one, according to the angle at which the light strikes the bright portions of the steel. When these portions throw the light they receive, out of the field, the former is the case, and the latter when they send it to the eye. When nicely illuminated each ray gleams with delicate iridescent tints, and the secondary star produced by the intersecting lines can be made to look distinct from the primary star, or as if it

were formed by the fan-spokes of the primary having the shape shown in Fig. 3, where each secondary spoke shows a knife edge near its outer margin, which thickens when the intersections begin, and broadens towards the centre of the disk. The transverse or nearly concentric markings arising from intersections are more striking than in the glass star, and exhibit an iridescence differing from that of adjacent parts.

The primary and secondary stars are easily made to look semi-transparent, as if those parts where the lines are thickest were composed of extremely fine silver gauze.

The most remarkable work which I have seen of Mr. Stanistreet's, is a star on steel, about half an inch in diameter, displaying 10 rays, each consisting of 12* bands of 40 lines each, making 4800 lines. Each ray has what Mr. Stanistreet calls a "serried edge" (as shown in Fig. 4), caused by the diamond point commencing each stroke a *very little* nearer to the centre, and so on in each band of 40 parallel lines. Then the next band of 40 lines is commenced at an angle of 1° 26′ 24″ from the preceding band, and ruled in like manner towards the centre of the star, and so in each of the twelve bands which constitute one ray of the star. These bands of *parallel* lines, by their mutual intersection at the above angle, give the wavy, or watered-silk pattern crossing each stellar ray, of which there are 10. In addition to the lines of the highly complex rays, there are groups of radial lines between each pair of rays, 250 in all. These radial lines, added to the 4800 parallel lines, make a total of 5050 lines in the whole pattern. The highly complex character of this star, the closeness of the lines composing the bands, and the very numerous intersections, give rise to very remarkable optical effects. Lit up with a silver reflector, the bands all stand up more or less vertically; the upper surface of these bands, or what seems such, is exquisitely watered when seen with a 3-inch objective, and the transverse bands produced at recurring distances by multitudinous intersections, look irregularly raised above the general surface, and the whole seems a fine tissue of glass threads, more or less iridescent. The secondary star produced by intersection is very striking in this specimen, and where two secondary rays intersect, a tertiary one will be seen. If a group of the radial lines is observed near the circumference of the star, they all look in one horizontal plane; but where they intersect the lines of the bands they look above or below, according to position and angle of illumination.

With a power of $\frac{2}{3}$rds and the useful vertical illuminator devised by the late Joseph Beck, the view which best satisfies the eye represents the lines as solid threads one under the other when simple, intersection takes place, and a tendency is created to view the spots of complicated intersection as higher than the rest. With a power of $\frac{1}{5}$th

* By accident the engraver has made 18 bands instead of 12.

and Powell and Lealand's modification of Professor Smith's vertical illuminator for high powers, the complex portions of the pattern are resolved, but with decided suggestion that the cuts are elevations, or threads laid upon a semi-transparent surface like white porcelain.

There is an advantage in studying the appearances that can be obtained with objects of this description, because the illusions can be corrected by higher powers and various modes of managing the light. They suggest causes of misinterpretation, and may thus prevent mistake, and the objects which are of great beauty illustrate a variety of diffraction effects.

On showing Mr. Stanistreet's exquisite work to several friends well acquainted with delicate mechanical operations, the remark has uniformly been, He must have costly and complicated apparatus to produce such results; but in reply to my inquiries, he writes, "My little ruling machine is a very homely and inexpensive affair, having been planned by myself, and constructed entirely by my own hands of such materials as came within my reach—crinoline wire, broken watch-springs, copper coins, and the heads of carpet pins, with some pieces of brass and steel, forming the entire structure." He adds that "the machine consists essentially of two separate parts: the first for giving equable motion to a minute fragment of diamond 'bort' set in the cleft of a piece of softened brass wire. This point is moved by a very fine steel screw of 100 threads per inch, which I made as perfect as my means admitted, and it moves the diamond through the agency of a simple lever $\frac{1}{1000}$th of an inch for each entire rotation of the screw."

"The disk of glass to be engraved is suspended over the diamond by a spring (crinoline wire), and is made to move across it by a revolving coin—a penny-piece, having an inclined plane of thin brass soldered to one-half its circumference—and this, when rotated, raises and develops the glass disk very gently, drawing it across the diamond through a space limited by adjusting screws, letting down the glass very gently and lifting it off suddenly at the end of each half revolution of the coin."

Mr. Stanistreet works very quickly with this machine. He is able to rule 100 parallel lines from 1—1000″ to 1—10000″ apart in one minute of time, but, as may be supposed, does not attempt very delicate work at such a pace. The complicated star last described—an exquisite specimen of delicacy and skill—occupied three hours and twenty minutes, which seems a short period for such a number of lines and so complicated a pattern.

I should add that the preceding description applies to the machine as first made. Before the specimens described were ruled, an addition was made to the apparatus with a more delicate means of motion than the screw. I am informed it would require accurate drawings to make the structure intelligible. Mr. Stanistreet informs me that

his last addition "enables him to rule lines at any required angle with the line of movement given to the diamond point; so that, assuming the latter to move—as in my machine—1 in 1000" for each rotation of the leading screw, I can rule lines closer to each other in the relation of the cosine of the angle from a perpendicular to the path of the diamond."

The glass or steel disk is rotated through any azimuth angle by means of a "worm-wheel" and endless screw.

In a note received after the preceding remarks were written, Mr. Stanistreet says, "I think I ought to mention a fact connected with the last specimen that I sent you. After completing the ten rays of the star I went on ruling another ray, supposing that I had still one to do, and I had ruled *three* supernumerary lines before my eye caught the index which told me that I had completed the circumference. I expect that the work would be marred by this excess, but on removing it from the machine I was unable to perceive any trace of irregularity, and it was only under the microscope that I found the three supernumerary lines occupying *almost* EXACTLY the site of the three first lines." The error is of no practical value; it would escape all ordinary notice, and serves to show the accuracy of the apparatus.

Mr. Stanistreet speaks most modestly of his machine, and of his work, as if it were easy. We may congratulate him on such a happy imperviousness to difficulty, and wish his further labours all success.

VII.—*The Fungoid Origin of Disease, and Spontaneous Generation.*

By JABEZ HOGG, Hon. Sec. R.M.S.

IN the report of the medical officer of the Privy Council just issued, the origin and pathology of contagion is ably discussed, and the crude hypothesis of Hallier bearing upon this point, who, it will be remembered, sought to prove that the microzymes and sporules of fungi which he found in the fluids of persons affected with cholera caused the disease and explained its contagious nature, is finally disposed of. This vexed question, one of no small importance to the public, and of great interest for the medical profession, receives at the hands of Dr. Sanderson, the writer of this part of the report, all the care and attention it really deserves. His experiments and investigations fully bear out all I have stated on this subject, and conclusively show that neither bacteria nor microzymes are concerned in the production of any specific form of disease in the living animal body, and therefore when found must be looked upon as an indication of a putrefactive process occurring after death. A

drop of water, a glass slide, or even a finger coming into contact with a fluid or tissue under examination, is quite sufficient to cause the development of either bacteria or microzymes, in an incredibly short space of time. In this way a disturbing element is introduced which mars and mystifies the most carefully made investigations of the histologist.

Admitting that the spores of fungi are always present in the atmosphere, although at some periods not in very great multitudes, it by no means follows, nor can it be shown that they are the cause of any specific form of disease. And, if it be true that so slight a contamination as that spoken of by Dr. Sanderson when brought into contact with a fluid is sufficient to change its character and start organic germs into life, then experiments said to prove that living matter can begin *de novo* in solutions subjected to long boiling must be accepted with extreme caution. For who can undertake to say with any degree of certainty that the breaking of a becker, in which a vacuum has been produced, can be conducted with sufficient care to prevent the possibility of a rush of air, carrying with it some organic particles, which shall contaminate or impregnate the whole? This, a point of the utmost importance, has not received much attention, although it is sufficient to embarrass and confound the results arrived at in the investigations of Dr. Bastian.

The ingenious way in which it is sought to explain experiments made by submitting a solution to a temperature of 160° F., alleged to be sufficient to destroy all evidence of life, while in another subjected to a much greater heat, ranging from 260° to 302° F., living creatures have reappeared, is by no means satisfactory. This admits of a different explanation, which will at once suggest itself to those who have thought over the phenomenon. Neither does it prove that because the elements of non-living matter are known to group themselves anew, so as to produce living matter under the influence of those physical forces which are concerned in bringing about the growth of a plant; that the same forces can be made to combine by long boiling to reproduce life or reconstruct the disintegrated particles of dead matter, and convert them into higher organisms than had previously existed. It seems to me impossible to attempt in this manner the settlement of a point of so much importance as that of the origin of life. And since we cannot undertake to say with anything like certainty that we have succeeded in destroying every living germ in any experiment we may institute, then, I fear, the spontaneous generation hypothesis is hardly worthy of further serious consideration. But with regard to Dr. Sanderson's investigations of certain contagious forms of disease, he produces positive evidence that nothing like bacteria or microzymes can be discovered in the blood of persons affected with scarlatina. This is an important

and interesting fact, one very suggestive as to the cause of particular forms of disease, and seeming to lead to the conclusion that contagious affections are produced by a putrefactive change, a contamination introduced from without into the circulation.

From whatever stand-point we view the important question of contagion, or the origin of life, I am quite sure it will ultimately end in a gain to our scientific knowledge; and as every additional contribution will I am sure be acceptable, I shall offer no apology for introducing a very interesting letter, written by Henry J. Carter, F.R.S., some four or five years ago, as a criticism on a paper of mine which appeared in the 'Intellectual Observer,' "On Phases in the Developmental History of Infusorial Life," a great portion of which is quite pertinent to the question under review at this moment,

BUDLEIGH SALTERTON, DEVONSHIRE, *March 14th*, 1867.

MY DEAR SIR,—I do not yet believe in spontaneous generation, nor will the theory, if ever substantiated, be so until a knowledge of the ultimate forms of the phenomena called "life" is obtained; while it seems to me that we are as far from this as from the ultimate atoms of matter.

When we see, under the microscope, insect forms almost as small as the smallest animalcules, and know, from inference, how complicated their structure must be; when we find their limbs as transparent as glass, and thus, apparently, as structureless, yet know that there is structure even in glass.

When we find that there is no extent to the slowness of change of form and movement in organized matter, that with the highest magnifying power possessed we can limit; that even unmelted iron is said to flow; when, on the other hand, the power of determining the velocity of bodies diminishes with the magnifying power, so that distance and magnitude itself are required to make us sensible of the rate at which comets travel, even if not of the presence of the atoms of matter *en masse* which form their nebulosities, so that neither one nor the other could be seen if close to us, any more than electricity or uncondensed steam.

When, I repeat, our perceptions in these respects remain so finite, how can anyone come forward with the assertion that there is such a thing as "spontaneous generation," based upon the presence of animalcules which, produced under any circumstances, may be, and probably are, far more complicated in their structure, and therefore higher in the scale of organic development, than a host of living beings with whose forms even we have as yet no means of becoming cognizant?

Progressive knowledge may lead the human mind to the beginning of vitality, to the quickening power of matter and its processes, but until this is reached, it seems to me premature to assume as a fact that there is such a power as spontaneous generation.

With reference to the next point in your paper, the transformation of the protoplasm of the vegetable cell into amœboid forms, who shall

limit the extent to which such forms may not penetrate into and live passively in the protoplasm of both animal and vegetable cells, until a favourable opportunity arrives for their further development? I, of course, include in the amœboid forms, the *Myxogostres*, now called by Du Bary "*Myxozoa*." Just before leaving Bombay, I found the brown stains in some cotton which was submitted to me for microscopical examination, to arise from the development of a mycelium originating in cells or germs of a mycetozoon, which were probably introduced into the cell of the cotton fibre when fresh, and which, on the moisture of the cotton during exposure to the rainy monsoon finding the vitality of its host extinct, naturally appropriated its protoplasm, and produced, while growing, the stains mentioned and consequent injury to the staple.

I am glad that we are at one accord as to the origin of protozoa in the cells of organized beings.

It appears to me that Dr. Hicks is in the same zone (so to write) of investigation in this respect as I was before I renounced my opinion of the "fancied" transformation of the vegetable protoplasm into amœboid animalcules. It was only after studying the *mycelizoa* fungi that I began to see the unlimitable extent to which such beings commencing their existence and even feeding themselves up to maturity, might enter into, and develop themselves upon the remains of their dead or dying host.

The contents of the root-like extremities, filamentous mycelium, and pin-head-like capsules of the *Mucorideæ* may issue, when their cellulose covering is ruptured, in the form of amœboid cells (that is, of course, as regards the *sporidia* before they are capsuled), and so creep away. Then the *Mucoridæ* are closely allied to the *Myxogostres* or *Mycetozoa*; and here no doubt the protoplasm of the *Mucor*-cell or filament, &c., issues in amœboid forms from its cellulose investment, which seems to be, as in many other instances, secreted by, and common to, a congeries of amœboid bodies, thus assuming the specific form of *Mucor*.

But there is no "transformation" here of the protoplasm, no perishing. The amœboid cells come forth at once and do not bore holes through the cell-wall as the *Mycetozoa* family when developed in the vegetable cell.

Hence, unless the protoplasm issues at once in an amœboid form, or forms, as a whole, as from the cell of *Œdogonium*, &c., or in plurality, as from the filaments and pin-head-like capsule of unmatured sporidia in *Mucor*, &c., I should still be inclined to view the product as not of the same, but of a different organism.

No doubt you saw the statement I last made of the probable reproductive process by impregnation in the Rhizopoda in the xvth vol. of the 'Annals,' p. 172.

I found in a pair of *Difflugia urceolata* (Carter) in zygosis, when crushed under the microscope, a number of monad-like monociliated, polymorphic bodies in active movement; the usual nucleated cells much larger; and apparently some of the latter which had become polymorphic or amœbiform.

The origin of the nucleated cells I had not been able to ascertain—that is, from what part of the *Difflugia* they come. That of the monad-like bodies I knew to have come from the nucleus, which frequently (under reproductive circumstances?) breaks up into these bodies, and therefore, in this case, was not present—had thus disappeared; while the amœboid bodies *without* cilium seemed to be but a more advanced state of the passive nucleated or ovi-like cells.

Thus I inferred that the small monociliated bodies coming from the nucleus were the male elements, and the larger nucleated cells the female elements, which, meeting together in the body of the parent, were, in their plastic state, thus brought together at the most favourable moment for impregnation, *i. e.* for the blending of the two elements. While the active amœboid bodies *without* cilium might have been the product after impregnation, thus prepared for independent existence when the parent might choose to throw them off, or might become effete and thus by dissolution allow them to escape into the water.

I have much more that I could state to you on the subject, but neither my leisure nor your patience, I fear, admits of the extension. Sufficient however has been written to show you the amount of interest I still take in these matters, and thus to prove to you how acceptable was the copy of your paper.

I am, my dear Sir,

Yours very truly,

HENRY CARTER.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Spectroscope in Microscopy.—Those who are interested in this peculiar department of the microscope will be interested in reading a very useful paper on the subject in Max Schultze's 'Archiv,'* published in May last. Messrs. Sorby and Browning have their earlier labours very fully recognized by the author, Herr S. Valentin.

The Anatomy of the Retina.—This is a subject which is taken up by Herr Max Schultze, who has a new paper on the subject in the May number of his 'Archiv.' It is illustrated by a very admirable plate, and is of considerable length. This work also contains a paper on micro-photography. It is illustrated by a couple of photographs, one of blood, the other of bone, which however do not reflect very much credit on the photographer, being in no way to be compared with Col. Woodward's efforts.

The Embryology of Scorpions.—One of the finest memoirs that have for years been published on this subject is the splendid essay

* 7 Band, 3 Heft.

of Dr. Elias Metschnikoff on this subject. It fills nearly 40 pages of Siebold and Kölliker's 'Zeitschrift,'* and is illustrated by four admirably choice plates. The development of the scorpion has been pursued from the very earliest stage of the ovum through all the series of changes by which it reaches the adult condition. These are very many in number, and deserve to be carefully studied by those interested in these animals.

Mitraria and Actinotrocha.—The above author has contributed also to the same journal as the above, a very valuable paper on these two species. He goes into the question of their development, and affords a very excellent plate in illustration. It is quite out of our power to produce such plates as these in this country.

Development of the Radiolaria.—Dr. W. Dönitz gives an excellent paper on this subject in the 'Archiv für Anatomie.'† The plate illustrating the author's remarks has been very carefully drawn.

Structure of the Chorda dorsalis.—A very long and important paper on this subject will be found in the 'Zeitschrift für Medicin,' &c.,‡ by Wilhelm Müller. Indeed the whole number, which is a special one, is by him, and it contains several very valuable contributions.

The Structure and Nature of Diatomaceæ.—This is an interesting paper contributed to the Vienna Academy by Dr. Adolf Weiss, and published with two plates in the 'Sitzungsbericht.'§ It deserves perusal by some of our microscopists devoted to minute structure.

Mediterranean Bryozoa, by Dr. Manzoni, is another good paper also in the same number of the Vienna Transactions.

On the Development of Plant-organs.—This is likewise a paper in the same number of the Vienna Journal. It is by H. Leitgeb, and is of considerable importance. It goes minutely into the question of what particular cells go to form special parts. In fact it relates to the development of the whole plant. There are four plates of engravings accompanying the paper.

NOTES AND MEMORANDA.

Royal Microscopical Society. Addresses Wanted.—It is believed that the following gentlemen no longer reside at the addresses given in the last edition of the 'Royal Microscopical Society's List of Fellows,' and as a new edition is in preparation, they are requested to forward to the Assistant-Secretary, Mr. Walter W. Reeves, King's College, London, their present address as early as possible. Should they be abroad and this request be not likely to come under their notice, perhaps some friend will be kind enough to give the required

* Band 21, Heft 2. † Reichert and Du Bois-Reymond, 1871, Heft 1.
‡ Band 6, Heft 3. § LXIII. Band 1 and 2 Heft.

information:—William Timbrell Elliott; James Robey; William Henry Spencer, M.A. Cantab.; John Shepherd.—King's College, *August* 4, 1871.

'The Lens;' a Quarterly Journal of Microscopy and the Allied Natural Sciences: with the Transactions of the State Microscopical Society of Illinois, is the title of a new journal which it is proposed to publish in Chicago, America. The State Microscopical Society of Illinois proposes to issue on the 1st of October next, the first number of a Scientific Journal, to be called 'The Lens.' While 'The Lens' will be devoted chiefly to the interests of Microscopical Science, no communication of value, relating to any department of Natural History, will be excluded. Without trespassing on the fields so ably occupied by 'Silliman's Journal' and the 'American Naturalist,' the only publications of like character in the United States, the pages of 'The Lens' will contain:—1. Original contributions consisting of papers read before some Scientific Society, or communicated directly to the Journal; 2. Original papers, elaborate or otherwise, illustrative of the *Natural History* of the Mississippi Valley, and the Far West; 3. A comprehensive *résumé* of the latest foreign inquiries, and critical reviews, with brief notices of the latest microscopical publications in this country and Europe; 4. Descriptions of all new forms of Microscopes and Microscopic Apparatus; and 5. Correspondence on matters of Histological controversy. Contributions requiring illustration will be accompanied by carefully-drawn plates, and the text will be printed on good paper in clear and legible type. 'The Lens' will be thoroughly scientific, advanced and comprehensive, and will be issued under the auspices, and in the interests of this Society. The size of page, 8vo; and each number will contain at least 48 pages of reading matter. Terms, 8*s*. per annum, *in advance*. In view of the fact that there are so few journals published in America, in these interests, the committee hope for the strong support, both contributional and financial, of all lovers of Science. Correspondence relating to the business management of the Journal should be addressed to Charles Adams, Secretary of the Publishing Committee, 1000, Michigan Avenue, Chicago. All other communications to the editor, S. A. Briggs, 177, Calumet Avenue, Chicago. *Officers of the Society:* H. A. Johnson, M.D., Pres.; S. A. Briggs, 1st Vice-Pres.; H. H. Babbock, 2nd Vice-Pres.; Geo. M. Higginson, Treasurer; O. S. Westcott, Sec.; Charles Adams, Cor. Sec.; E. H. Sargent, Charles Biggs, Charles Adams, *Publishing Committee*.

CORRESPONDENCE.

Observations on Surirella gemma.

Dear Sir,—The fact should not be lost sight of with regard to the interpretation of the markings on *Surirella gemma*, that Mr. John Mayall, in his paper "On Immersion Lenses and Test-Objects," pub-

lished in the Royal Microscopical Society's 'Transactions,' February, 1870, vol. i. p. 92, points out Hartnack's error, and observes, in place of elongated hexagons represented by this optician, and copied without comment by Dr. Carpenter, the frustule "is analogous to that of the *Grammatophora subtilissima*, and with Hartnack's immersion $\frac{1}{10}$th this latter frustule" appears to be similar to *G. marina*, or *P. angulatum*. Mr. J. Mayall subsequently convinced Hartnack of the error into which he had fallen, and concludes his paper with these words, "*Surirella gemma* may truly be called a touchstone for a high-power objective." I should add that Dr. Woodward's admirable micro-photographs of this and other frustules, are more nearly perfection than anything I have ever seen.

Yours, &c.,

J. Hogg,

Hon. Sec. Royal Microscopical Society.

PROCEEDINGS OF SOCIETIES.*

Biological and Microscopical Section of the Academy of Natural Sciences, Philadelphia.

At a stated meeting held April 3rd, 1871, the Director, S. Wier Mitchell, M.D., in the chair,—

A donation was received from the Surgeon-General's office at Washington of Colonel J. J. Woodward's interesting report, entitled "A Memorandum of the Test Podura," with five photo-micrographs.

Dr. James Tyson exhibited slides of the deposit from two specimens of urine from a case of so-called *intermittent hæmaturia*, which were interesting, if not important, from the fact, that the first specimen, though containing granular casts, did not contain blood corpuscles, and that the second, between the discharge of which and the first the urine had become quite clear, contained, in addition to granular casts, blood corpuscles and blood casts.

The importance of this observation lies in the circumstance that in the cases of intermittent hæmaturia reported by Harley† blood corpuscles were exceedingly rare, being found in a single case, and not more than one or two in the field of the microscope. So rarely, indeed, have corpuscles been present, that Dr. Beale, in the first volume of 'The Practitioner,' August, 1868, says: "It is, therefore, improbable that in these cases there is any hemorrhage, as in acute inflammation of the kidney, and they ought not to be spoken of as cases of hæmaturia."

* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

† 'Medico-Chirurgical Transactions,' vol. xlviii., 1865.

In the present case all the other phenomena of intermittent hæmaturia attend; and in the second specimen of urine there were many free blood corpuscles and blood casts; while in the first the most careful searching detected none.

The treatment found most useful in intermittent hæmaturia—that by anti-periodic doses of quinine, preceded by a purgative dose of calomel—has here also been the most satisfactory, there being no recurrence since its adoption, although four weeks have now elapsed, while other modes of treatment adopted since October, 1870, when the affection first appeared, have signally failed.

Dr. J. G. Richardson exhibited a slide charged with pulmonary elastic tissue from the boiled sputa (according to Dr. Fenwick's method) of a phthisical patient in the Episcopal Hospital, and called the attention of the Section to two characteristics of its elastic fibres: first, the delta (Δ), rather than simple Y-shape frequent among the fragments, which he attributed to the greater resistance, at the meeting point of the walls of three air-vesicles, to any disintegrating process; and second, the transverse fracture of its component elastic filaments, resembling that of an india-rubber thread, instead of a frayed-out appearance, similar to that presented at the extremity of a broken cotton or linen string.

By these peculiarities pulmonary elastic tissue can generally be distinguished from objects which occasionally counterfeit its aspect, as, for example, folds in the walls of boiled starch corpuscles, mycelial threads of fungi (which, when dichotomous, often have stem and branches of nearly the same diameter), and vegetable fibres, which seldom break transversely, and which, when split, generally assume the Y and not the delta shape.

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Grundriss der Physiologie d. Menschen. Prof. Dr. Karl Vierordt. Tübingen. Laupp.

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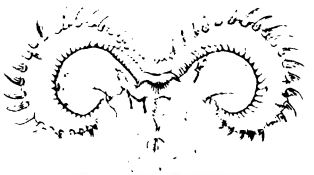
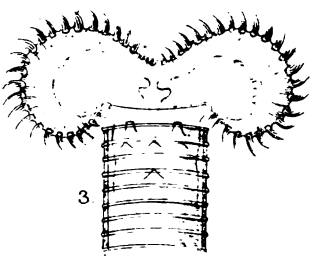
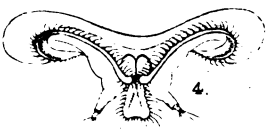
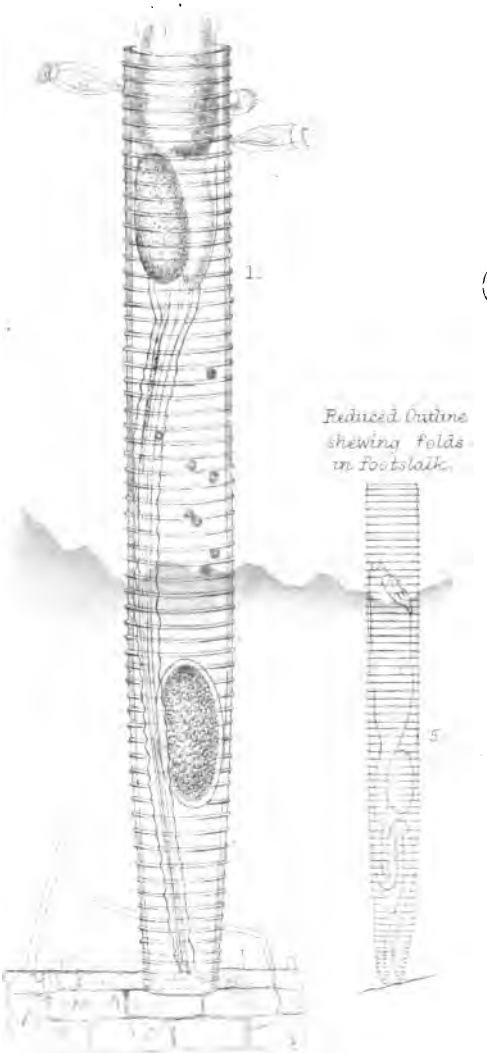

Neural aspect.



Hæmal aspect.



Anterior aspect with marginal Cilia suppressed.



Frontal region before emergence of Corona.

C. Cubitt delt. W. West & Co^s imp.

Melicerta Cubitti.

THE

MONTHLY MICROSCOPICAL JOURNAL.

OCTOBER 1, 1871.

I.—*A Rare Melicertian; with Remarks on the homological position of this Form, and also on the previously-recorded new species Floscularia coronetta.** By Charles Cubitt, F.R.M.S.

Plate XCVIII.

Introductory.

Occupied in the prosecution of certain investigations on the homological position of the members constituting the so-called Families of Ehrenberg's Class Rotatoria, with the view to the establishment of a correct natural Classification as the basis of a work on which I am at present engaged, I find myself fortuitously placed in command of a field of operations in the instance of a small streamlet, a tributary of Northbourne Brook, in Kent, overgrown with the bladder-wort (*Reticularia*), which streamlet is literally swarming with every member of the following genera of Ehrenberg's system, viz. *Stephanoceros, Floscularia, Melicerta, Tubicolaria, Limnias,* and *Œcistes,* together with several of the fresh-water polyzoa; and amongst the former I find a rare Form, or at all events one which has hitherto escaped the notice of observers generally; and with the object of establishing the correct position of this elegant Form as well as that of *Floscularia coronetta,* it is incumbent on me herein to review briefly the homologies of the particular Families in which they will respectively be placed.

Terminology and Aspects.

The Terminology and Aspects which have hitherto been employed, do not apply appositely to the several Forms embraced; the term *lorica,* for instance, signifying in its literal interpretation a coat of mail or an armour, is illogical and absurd when indiscriminately used to represent the simple hyaline investments of the solitary *Flosculariana,* the clustered forms of the *Lacinulariana,* or even the compound sheaths of the *Melicertians.* I have therefore adopted certain modifications which will be employed throughout these remarks; and for *lorica* we shall substitute the term *Vagina.* The word *disk,* as signifying alike the region of the tentacular crown of

* 'M. M. Journal,' September, 1869.

the *Floscules* and the ciliated lobes of the *Melicertians* is equally absonant, and for this we shall substitute and henceforth apply the term *Corona*. And following a good example* in the matter of aspects, we shall employ *Neural* to indicate the ganglion side and *Hæmal* the opposite, and further, shall characterize the active vibratile appendages of the corona as *Cilia* in contradistinction to the *Setæ*, which perform an intermittent action when investing the corona; and for want of a more appropriate term we must apply this also to the delicate hairs which furnish the pistons of the antennæ.

Stephanoceros Horatii.

The obvious diversity of the form and position of the corona of the *Floscularians* clearly and indisputably separate them from a Family association with the *Melicertians* in which the corona subtends the mouth; and moreover, while the members of the former manifest a true bilaterality by the preponderance of the neural margin of the corona, no such bilaterality obtains either with *Stephanoceros* or with the miscalled *F. coronetta*. The corona of the *Floscularia* is, as generically characterized in reference to the lobes, *short, broad, knobbed, expanded;* while the tentacles of both *Stephanoceros* and this species are *long, slender*, and *erect;* we see further that the corona of the Floscules is, with one exception, beset with setæ upon the lobes only, while the whole margin of the corona of *Stephanoceros* and of this species is invested in one continuous series. We see also that these two Forms are identical in other points of their economy, and are of a higher type of organization than that of the *Floscules*, first as regards their nervous, muscular, alimentary systems, and further in the fact that the stage which supports the tentacles of *Stephanoceros* and *F. coronetta* acts in the double capacity of the *calix* and *lophophore* of the Fresh-Water Polyzoa, and like these Forms they possess an *epistome*, which is situated just above the ganglion, they both manifest an organic attachment of the tunicle, and perform palpable invaginations of what may, for the sake of argument, be termed the *endocyst* with every retraction of the corona—points which establish a homological identity with the so-called *F. coronetta*, and *Stephanoceros Eichornii*, and as such it will in future be denominated *Stephanoceros Horatii*.

THE FAMILY MELICERTADÆ.

We now come to a consideration of the *Melicertians*, and with these we find that the corona subtends the mouth, that, whether simple or serrated, its periphery is continuous and unbroken, and is furnished with an uninterrupted range of active vibratile cilia,

* Allman's 'Fresh-water Polyzoa.'

whose procession, or the appearance of such is seen to be in one and the same direction from left to right when viewed in a neural aspect, and that it is supplemented by a secondary range of more minute cilia, whose procession is divided into two channels, emanating from the under-side of the corona in its expanded condition, and progresses, so to speak, in opposite directions, conveying the captured particles to the mouth, where they are subjected to the scrutiny of a bilobed ciliated organ, situated immediately above it, though, unlike the epistome of *Stephanoceros*, it is placed in a hæmal position, but it exercises a similar function in selecting the nutritive particles from the general mass, and in rejecting others, as the scrutiny may determine. This secondary range of cilia obtains, in a more or less developed condition, in every individual member of the *Melicertians*, whether it be *Limnias*, *Œcistes*, or *Lacinularia*, and it terminates in a process projecting from the ganglion side, and in most, but not all of the members, it is supplemented by a fabricating organ, which is employed in the construction of their compound tunicles, an exceptional instance occurring in a little *Melicertian*, which employs her voided excremental pellets to cover the otherwise hyaline investment. This fabricating organ, in its *normal position*, is also wanting in the instance of the elegant Form which has induced these remarks, Plate XCVIII., Fig. 1, and in bringing them to a close it will be only necessary to add that the genus *Melicerta* must include all those members in which the foregoing points are permanent, and that *Limnias*, *Œcistes*, and *Tubicolaria* are essentially *Melicertians*, and will be referred to as such; retaining, however, their hitherto specific appellations as for the sake of facility of recognition.

Melicerta annulatus.

My attention was first drawn to a vagina of this species, which at a glance exhibited proportions differing from any hitherto known Form, containing within it an ovum of a magnitude equally surprising. I at once applied the micrometer, and for the moment failed to notice the corrugations which, singularly enough, coincide exactly with the 2000th divisions of the micrometer; they occur as little ridges formed around the circumference of the vagina, which in all young, and young adults is perfectly hyaline, manifesting a decided and brilliant orange tint, but seen at the two sides when in proper focus they are rendered very distinct, the orange tint becomes condensed into a deep carmine. How these ridges become formed with such marvellous precision is a matter that must strike all with wonder and admiration; and although I do not feel myself prepared at once to state anything definite as to their formation, I can only suggest it as worthy of attention that the anterior regions manifest a considerable and somewhat complicated

departure from those of the other Melicertians, to which we shall give some consideration in a subsequent paragraph.

The footstalk is most frequently wrinkled, and it manifests very distinct muscular bands, see Plate XCVIII., Fig. 1; but notwithstanding the presence of these, they do not produce sufficient contraction of the stalk to enable the animal to withdraw itself within the vagina without producing folds in the footstalk, as shown by the rough sketch, Plate XCVIII., Fig. 5. The integument, which is corrugated all along the footstalk, ascends to the corona, where it forms an orifice into which the corona becomes retracted by an invagination, the line of demarcation being very distinct even during the eversion of the corona, leaving, however, the two respiratory tubes the most prominent objects in view, as is the case with every one of the *Melicertians*, whether these tubes are rudimentary, or otherwise highly developed.

In its retracted condition, Plate XCVIII., Fig. 2, the corona manifests two distinct projecting processes beyond the setiferous tubes which, though they present the same general appearance, are not provided with setæ, but manifest at their extremities a bright red spot under the illumination of the Wenham parabola; we do not expect to find eyes in a hæmal aspect as these spots are situated; beneath them there are three other processes which are less highly developed, and the distance between these two upper processes and the three lower ones corresponds precisely with the "pitch," or distance apart of the annulets of the vagina, and we see that although the corona is frequently protruded far above the margin of the vagina, it never remains in that position for periods of long duration, except in the instances of old animals, whose vaginæ are coated with extraneous matter, but with younger animals, the corona on the act of eversion is seen to emerge somewhat above the margin, and then to subside to such a position that these two processes become identical with its margin, suggesting the not unreasonable notion that these peculiar organs are employed in the act of constructing this delicate investment, but further investigation is necessary to confirm this supposition; it certainly seems somewhat inconsistent to find that while in every other member of the Family the fabricating organ is situated on the neural side, it should in this particular instance be placed on the opposite; but if their capacity as such be truly surmised, they certainly produce an effect differing essentially in the nature of the vagina so produced, which in this instance is more brittle than viscid.

While hastening for the press, I must conclude these observations with a cursory notice of the action of the marginal cilia, which, from the tardiness of their action in this particular Form, offer every opportunity for correctly appreciating their action, and of representing them on a drawing, though it is of course impossible

to convey the appearance of objects in motion; however impressive may be their appearance, the onward procession is but an optical illusion; I assume therefore that for the instant they are fixed when by the employment of *linear projection* the individual positions of the cilia are found to be as indicated on the drawing, which I submit is correctly represented, and shows more clearly and truly the actual appearance assumed in life.

II.—*On an Improved Method of Photographing Histological Preparations by Sunlight.**

By J. J. Woodward, Assistant-Surgeon, U. S. Army.

General: In January, 1870, I had the honour to submit to you a report in which I detailed the results of a series of experiments, which showed the superiority of the Electric and Magnesium lights over sunlight, as heretofore employed, for the production of photo-micrographs of the soft tissues. In June of the same year I made a report in which I showed that similar results could be obtained with the Oxy-Calcium light. With these various artificial sources of light I obtained pictures which appeared to me to be "clearer and better defined than any photographs of similar objects I had hitherto seen produced by sunlight."

So many cloudless days are offered to the photographer in Washington that I could not but regret these results; yet they appeared to be final at the time of writing. During the last few months, however, I have found improved methods of using the light of the sun for photographing the soft tissues, and have arrived at results which must materially modify the conclusions of my former reports.

Not that I have anything to withdraw from the opinions I have expressed, as to the certainty and success attending the use of artificial lights for the purpose named, but I have much to add with regard to the most advantageous methods of using the light of the sun for obtaining satisfactory pictures of tissue preparations, and such other objects as approximate them in optical characteristics.

If a well-made preparation, of some normal tissue, or of some pathological growth, stained with carmine, silver, or gold, and mounted temporarily in glycerine, or permanently in Canada balsam, be illuminated by white cloud illumination, or by lamplight, and found to be all that could be desired, it will nevertheless appear very unsatisfactory if illuminated by the direct rays of the sun.

The eye glancing through the tube of the instrument, dazzled by the powerful light, discerns amidst the blaze innumerable coloured

* The report is made to Brigadier-General J. K. Barnes, Surgeon-General U. S. Army.

rings, produced by diffraction and interference, which disturb the normal appearances of the preparation and render its interpretation impossible.

If the image be received upon a white screen similar phenomena obtrude themselves, destroying the clearness of the picture, though no longer injuriously affecting the eye; and if monochromatic light is employed, although the disorderly play of colour disappears, black rings and lines of the most manifold character and direction take their place. Pictures produced under these circumstances are of course quite useless, and the difficulty occurs not merely in the case of tissue preparations, but in a very large number of other objects.

To escape these disagreeable results it has heretofore been the practice to pass the solar pencil through a piece of ground glass. This plan is recommended in all the treatises on photo-micrography, and has hitherto been employed in the solar work done at the Army Medical Museum. The method is effectual in getting rid of the diffraction and interference phenomena complained of; an image is obtained which is clear and satisfactory to the eye looking down the tube, but it appears very weak on the screen and is sadly deficient in contrast. These faults are reproduced in photographs of objects thus illuminated, and, moreover, the time of exposure is enormously increased. Such pictures are decidedly inferior to those which can be obtained by the Magnesium, or even by the Calcium light, with which no ground glass is used.

I desire now to call your attention to the fact that in the course of some recent experiments I have ascertained that the diffraction and interference phenomena above complained of, may be prevented by the use of a suitable condensing lens, even better than by the ground glass; that by this plan the exposure may be greatly diminished, say from three minutes for five hundred diameters, to a fraction of a second, and that the resulting pictures are not merely quite as free from diffraction and interference phenomena as the best that can be obtained when the ground glass is used, but are characterized by greater contrast and superior sharpness of definition.

The details of my new method are as follows:—The microscope being placed on a shelf at the window of the dark room, and its body made horizontal, the achromatic condenser is illuminated by a solar pencil reflected from a heliostat upon a movable mirror outside the shutter and thence into the dark room, precisely as described in my original paper on photo-micrography.* No ground glass is used, but instead a lens mounted in a suitable tube is fixed in the opening of the shutter through which the solar pencil enters. This lens is an achromatic combination about two inches in transverse

* 'American Journal of Science and Arts,' September, 1866.

diameter and of about ten inches focal length. It is placed at such a distance from the achromatic condenser that the solar rays are brought to a focus and begin again to diverge before they reach the lowest glass of the achromatic condenser.

For anatomical preparations requiring for their display from two to five hundred diameters I use a $\frac{1}{8}$th of an inch objective, without an eye-piece, obtaining the precise power desired by variations in the distance of the sensitive plate from the stage of the instrument. I have lately given the preference to immersion objectives, the corrections of which I find are generally well suited to photographic requirements.

Now, with a $\frac{1}{8}$th objective and the arrangement above described, the field is so brilliantly illuminated that the eye cannot safely be permitted to look down the tube. The image is therefore received on a piece of white cardboard, and sitting by the microscope to make the adjustment I view the card with both eyes precisely as in the case of the ordinary solar microscope. With these arrangements, the cardboard placed from two to four feet from the stage of the microscope is sufficiently well illuminated to permit distinct vision, even when objectives of the shortest focus are used and powers of five to ten thousand diameters obtained. While the object is thus seen on the white screen in its natural colours, the cover corrections, focussing, management of the achromatic condenser, and selection of the portion of the preparation to be photographed, are readily managed. When all is satisfactory I insert an ammonio-sulphate cell between the large lens and the achromatic condenser, and draw down the velvet hood which prevents leakage of light from about the microscope into the dark room; then going to the plate-holder I make the final focussing in the usual way on the ground glass, or on plate glass with the help of a focussing glass, according to the nature of the object.

With powers of five hundred diameters or less I at first experienced some difficulty in giving the right exposure; for as the time required was but a fraction of a second it was a matter of some difficulty to regulate it with precision. At length I succeeded by arranging a sliding shutter, with a transverse slit of variable width, so adjusted as to fall with its own weight before the tube of the microscope, the exposure being made during the passage, and the time of exposure regulated by the width given to the slit.

Of course it occurred to me that for such short exposures the heliostat might be dispensed with, and I found on trial without it that a large right-angled prism used in the position of total reflexion, or even an ordinary mirror, gave excellent results; the exposures being even shorter than when the heliostat was used, since there was but a single reflexion. I could not satisfy myself, however, that the quality of the pictures differed from those

obtained with the help of the heliostat, except perhaps that in certain cases the prism seemed to offer advantages which will be referred to hereafter. Under these circumstances the heliostat appears desirable for ordinary use, since the solar pencil being thrown in a constant direction, the trouble of adjusting the illumination of a series of objects is considerably diminished, but I have convinced myself by trial that equally good pictures can be produced without it, even with very high powers, a circumstance of considerable interest where motives of economy preclude the microscopist from procuring this convenient instrument.

A few remarks with regard to certain points in the procedure above sketched seem called for.

First, with regard to the selection of objectives suitable for photographic work of this kind. The power of the objective to be used will depend of course upon the details it is desired to display. I find it best to use the naked objective without eye-piece or amplifier, and not as a rule to fix the sensitive plate more than three or four feet from the stage of the microscope. A $\frac{1}{8}$th objective may be conveniently employed to obtain powers of from two to five or six hundred diameters, a $\frac{1}{16}$th for higher powers up to twelve or fifteen hundred diameters. Suitable amplifiers or even eye-pieces may be used in either case, with great increase of the magnifying power, and often with admirable scenic effect, but there is always a certain loss of definition. Still such amplifications may sometimes be advantageously resorted to, especially in the case of objects which present very minute details; for in these cases the paper prints will often lose many of the fine details of the negative, and the loss of definition incurred by the amplifier or eye-piece is not unfrequently less than that encountered in attempting to transfer to paper a negative prepared with insufficient magnifying power. Thus far my experience is decidedly in favour of using sufficient power in the first instance, rather than attempting to enlarge negatives taken with less power.

The objective selected should of course be unexceptional in defining power, and should always be specially corrected for photography. It has been erroneously stated by Moitessier* that if an ammonio-sulphate or other blue cell be interposed in the solar pencil, all special corrections of the objective may be dispensed with. This proposition, which has been adopted by many other writers, appears plausible, but a little consideration will show it to be quite erroneous.

Everyone knows that a good objective must be free from spherical as well as from chromatic aberration. Of course the use of monochromatic light disposes of the chromatic trouble. Not so with the spherical aberration. Now this aberration, like the

* 'La Photographie Appliquée aux Recherches Micrographiques.' Par A. Moitessier, Paris, 1866, p. 180 *et seq.*

chromatic, is corrected mainly by the just combination of flint with crown glass in the several pairs which constitute the objective. If these are so adjusted as to correct spherical aberration as nearly as possible for white light, they will no longer do so for light which has passed through the ammonio-sulphate of copper. Until objective makers take this fairly into consideration the microscopist who desires to photograph what he sees is left to a happy chance in the selection of his objectives. For even those makers who profess to prepare objectives "specially corrected for photography" do not deal any too well with the problem. If they would test their objectives, while making them, with violet light, we should have better results; for with such illumination the eye can see all that photography can execute and no more.

But this circumstance fortunately enables the microscopist to select from the objectives in the market those which are suitable for photography. It is only necessary to test their performance when illuminated by sunlight which has passed through an ammonio-sulphate cell. Now it fortunately happens that the high-power immersion objectives of certain makers, especially those of Powell and Lealand, possess very nearly the corrections which theory would indicate as best adapted for photographic use. Nevertheless it can hardly be doubted that even these objectives could be greatly improved if the makers would take into consideration the principles involved in the foregoing remarks.

A second point, which deserves attention, is the use of the large condensing lens above described. This lens, it will be understood, corresponds with the large condensing lens of the ordinary solar microscope, while the achromatic condenser takes the place of the so-called field-glass of the same instrument. It has already been mentioned that this lens should be placed at such a distance from the achromatic condenser that the solar rays may be brought to a focus, and begin again to diverge before they reach its lowest glass. A different arrangement is usually employed in the solar microscope, the field-glass being placed at such a distance from the first condenser that the solar rays impinge upon it before they come to a focus. As a consequence, the convergent pencils proceeding from the first lens are still further converged by the field-glass, and a burning focus of heat, as well as of light, is produced, which is damaging to the preparation as well as to the balsam cement of the objectives used. If, however, the rays from the first lens are permitted to come to a focus and to begin to diverge before striking the second, this latter can readily be adjusted so as to bring the illuminating rays to a handsome focus, while the heat rays, after passing the second lens, become parallel or even divergent according to the position of the achromatic condenser, and all trouble from the solar heat is thus completely avoided. So successfully may this

separation be effected, indeed, that I have frequently obtained light enough to give distinct vision and admirable definition on the cardboard screen with five thousand linear diameters or even higher powers (obtained by the immersion $\frac{1}{16}$th, an amplifier, and four feet or greater distance), while the heat was so slight that the drop of water used with the immersion lens did not require renewal oftener than about once in two hours.

I had employed this device for several months, and supposed it to be quite novel, when I read the paper of the late distinguished President of the Royal Microscopical Society of London, the Rev. J. B. Reade, "On the Separation of the Rays of Heat from the Rays of Light in Solar and Oxy-hydrogen-gas Microscopes."* I learned from that article that Mr. Reade had devised this very plan as an improvement to the solar microscope as long ago as 1836. The advantages attained may be stated in his own lucid words:—

"It is evident by this arrangement of lenses we convert the parallel solar beam first of all into a cone of light-giving rays within a cone of heat-giving rays, and the principal focus of heat is farther from the condensing lens than the principal focus of light. But after these rays cross the axis we have, conversely, an equal and opposite cone of heat-giving rays within a cone of light-giving rays, and a plano-convex lens or hemisphere, if placed in this second cone at the distance of its own focal length from the principal focus of heat, will be at a distance greater than its focal length from the principal focus of light; and consequently the rays of heat, after passing through this lens, will become parallel, while the rays of light converge to a second focus. I have approximately measured the heating power of the thermal rays of the second cone when rendered parallel by the plano-convex lens, and I found in the month of December that the mercury in a sensitive thermometer, when placed in the second focus, did not reach 90° Fahr., while at the same time the heat at the focus of the first cone was sufficient to discharge gunpowder."

Mr. Reade appears to have experimented with low-power objectives only, for he speaks merely of such preparations as the head of a flea. He therefore succeeded very well by using a single lens in his field-glass. With such powers as the immersion $\frac{1}{8}$th and $\frac{1}{16}$th I find it better to use an ordinary achromatic condenser instead. The principles involved are of course identical. For the first condenser also, I have been using an achromatic combination of the dimensions and focal length above mentioned, taken from the back of an ordinary photographic portrait tube, but I am not sure that a simple plano-convex lens of the requisite diameter and focal length would not answer every purpose.

* 'The British Journal of Photography,' December 16, 1870, p. 590.

The introduction of the ammonio-sulphate cell would of itself prevent the passage of most of the heat rays falling upon it, but if this were the only means of excluding them it would not be possible to focus primarily with white light on the cardboard screen in the manner which I have found so convenient.

I have already stated that the time of exposure required for the production of pictures magnified five hundred diameters or less, was a fraction of a second. With higher powers it increases, varying with the management of the achromatic condenser. For four thousand diameters I have sometimes needed as much as twenty-five seconds.

So long as the exposure is greater than a second, the requisite time may readily be given with a piece of velvet, or a cardboard screen held in the hand. For shorter exposures some mechanical contrivance is indispensable. That alluded to above seems to answer every purpose, and is arranged as follows:—A wooden screen is fixed between the microscope and the sensitive plate, as close as convenient to the microscope. To prevent side lights reaching the plate, the screen is connected with the window-shutter by velvet curtains, which can be turned aside to manipulate the instrument, and be let down at the proper time. A circular hole, three inches in diameter, is made in the screen opposite the tube of the microscope for the transmission of the image. In front of this a light shutter slides loosely up and down, held in place by a cleat of wood on each side, the design being to permit the shutter to fall edge foremost with as little friction as possible. The shutter may be made of thin metal, of wood, or even of cardboard. I am using one of pine wood the $\frac{1}{16}$th of an inch thick; I have used one of cardboard with equal success. In the shutter is an opening, three inches wide by ten long, covered with a cardboard slide, by means of which any width of slit, from a fraction of an inch to ten inches, can be given. The part of the shutter below the slit closes the aperture through which the image passes when the shutter is fixed in place before the exposure is made. On drawing a wooden trigger the shutter is started on its fall, which is arrested by a piece of string of suitable length. The exposure has now been made, but the aperture through which the image passes is again closed, this time by the part of the shutter above the slit. The shutter is so light that the jar caused by the sudden arrest of its motion by the string is too trifling to do any damage to the microscopic apparatus, and as it occurs after the exposure is over it cannot affect the image. I find that if when the shutter is started the lower edge of the slit is an inch above the aperture through which the image passes, a convenient velocity is attained for a magnifying power of two to five hundred diameters, arranged as I have described. For still shorter exposures, necessitated by lower powers or other circum-

stances, it would be best to start the shutter from a greater height, which would give greater velocity to the passage of the slit, and any available fraction of time desired might thus conveniently be obtained. The whole arrangement is inexpensive, and may be manufactured in a few hours by anyone, out of a deal board, a few pieces of cardboard, and a yard or two of cotton velvet.

Of course the fractional measures of time obtained in this way are not absolute, since the friction must be variable, unless the apparatus were made in a more costly manner of metal. But I have found that the variations thus introduced are so small that they may be disregarded, and that while the starting-point remains the same, the width of the slit in the falling shutter indicates fractions of time which may confidently be counted upon to give proportional photographic results.

The next subject for remark is the arrangement employed when the heliostat is dispensed with.

For this purpose the contrivance usually employed for the solar microscope answers very well. A circular disk of brass, with toothed edges, is let into a square plate of the same metal, and is turned by a small toothed wheel, to which a suitable button or milled head is attached. Through the centre of the disk passes a tube six or eight inches long and two inches in diameter, the outer extremity of which is fitted to receive the large condensing lens. Just below this tube an arm is firmly attached to the outer surface of the disk for the purpose of carrying the mirror or right-angled prism, to which any desired inclination can be given by a rod passing through the disk by the side of the tube. The whole arrangement is quite like the similar parts of the ordinary solar microscope, and hence needs no minute description; it is fitted into a window-shutter, which must of course face to the south, and the room being darkened the motions of the mirror or prism can readily be controlled from within. If the condensing lens is used I do not think any material advantage can be obtained from the prism, and its expense is a decided objection. In the winter season in this latitude a prism of over five inches hypothenuse is required, and its cost is a serious item. An ordinary glass mirror answers, I think, quite as well for the tissues and most other purposes. There are, however, certain objects, such as the Pleurosigmata and some other diatoms, the Nobert's Test-plate, and the scales of certain insects, for which the condensing lens is unnecessary. The achromatic condenser, illuminated by a parallel solar pencil, answers better in these cases, and if it is properly managed no diffraction or interference phenomena are produced. I am satisfied that in such cases the pure parallel pencil obtained from the prism gives better definition to the image than can be obtained by the double pencil reflected from an ordinary glass mirror. A mirror silvered on the reflecting

surface would, I suppose, answer the same purpose; but such mirrors are not permanent, and are troublesome to keep in order while they last. Moreover, if the prism is used only for this purpose, a very small and cheap one will answer, since a pencil half an inch in diameter is all that is required. Such a small right-angled prism is furnished with most large microscopes, and can readily be mounted outside the brass disk so as to answer the special purpose indicated. For all those objects which require the large condensing lens to avoid diffraction and interference, a common glass mirror will answer well enough. For lower powers than two hundred diameters, however, the ordinary mirror will often be found to reflect too much light, and the image on the cardboard screen will be found too brilliant to be conveniently observed for any length of time. In such cases a piece of plain unsilvered plate glass may be substituted for the mirror. The greater portion of the solar light passes through it and is lost, but enough is reflected to make pictures of four hundred diameters in from two to three seconds exposure, and these pictures have all the qualities of those made with ordinary mirrors. I have tried instead to diminish the light by absorbing a part, using for this purpose an ammonio-sulphate cell of considerable thickness, but find that this plan diminishes the contrast and definition of the image, which is not the case when a mirror of simple plate glass is used as above described.

With regard to the management of the plate-holder, the apparatus for focussing, and other accessory arrangements, I need only say that I employ for the solar light the same simple plan which I have described in full in my reports on the use of artificial lights in photo-micrography.

Since making the experiments which have led to the foregoing results I have modified my method of dealing with the electric light in photographing the tissues. I first render the divergent pencil proceeding from the carbon points as nearly parallel as possible by means of the condenser, usually supplied with electric lamps for this purpose, and then introduce into the parallel pencil, instead of a ground glass, the very same condensing lens described above for the process with solar light. The image is received primarily on a cardboard screen, and the remaining details do not differ from what has been related above. The time of exposure does not exceed a single second for four hundred diameters, and the sharpness of the pictures exceeds any of my former results. Indeed, with this new arrangement I must say that the electric light appears to me to retain the apparent superiority over sunlight, remarked in my paper on the use of this method of illumination in photo-micrography, at least in the case of all those objects which in themselves possess but little contrast. For well-made tissue preparations, however, I find the best work I can do with the electric light, so similar to the best

attainable by sunlight, used as above described, that I should rarely take the trouble to set up the battery and work the electric lamp, unless it was desirable to work at night or in unfavourable weather.

It only remains to append some examples of the results attained by sunlight employed in this manner. In selecting a few negatives for this purpose I have preferred to confine myself to those which represent normal tissues, magnified to the moderate extent of four or five hundred diameters. I have done so because I believe that the greatest practical results are to be anticipated from the reproduction of similar objects with like powers. I would refer those who are curious as to the possibilities with higher powers, to the photographs accompanying my "Memoranda" on the Test Podura, and on *Pleurosigma angulatum* and *P. formosum*, the majority of which were produced by the methods above laid down. Among the views will be found one of *P. angulatum* and one of *P. formosum*, each magnified 4500 diameters. The following is a brief description of the subjects represented in the photographs which accompany the present paper.

[The following photographs have been received by us, but of course are not reproduced, being representations of objects familiar to most anatomists. They are, some of them, very correctly done; but we fear they do not yet represent all that is seen with the eye. Some of them, as for example Nos. 3, 4, 5, are, we fancy, rather imperfect representations of the objects they are intended to represent.—Ed. 'M. M. J.']

No. 1. Photograph representing a bundle of the striated muscular fibres of the mouse. Magnified 500 diameters by Powell and Lealand's immersion $\frac{1}{8}$th. Negative No. 368, New Series. From preparation No. 2338, Microscopical Section. The preparation was made by Dr. J. C. W. Kennon. The capillaries were injected with carmine, and the fragment selected is mounted in Canada balsam. For photographic purposes the focal adjustment was arranged to display the transverse striæ, and hence the capillaries appear somewhat out of focus.

No. 2. Portion of the peripheral wall of one of the alveoli of the lung of a frog. Magnified 500 diameters by Powell and Lealand's immersion $\frac{1}{8}$th. Negative No. 365, New Series. From preparation No. 3639, Microscopical Section. The preparation was made by Dr. E. M. Schaeffer, one of the assistants in the Microscopical Section. The vessels were injected with a dilute silver solution, and the preparation, after staining with carmine, was mounted in Canada balsam. The photograph represents a small artery breaking up into a network of capillaries. The cells of the endothelium are mapped out by the silver, and the carmine-stained nuclei appear in many places. This picture is introduced for comparison with the best of those obtained with artificial light appended to my report on the Histology of Minute Blood-vessels.

No. 3. A section of the eyelid of a calf, showing sebaceous glands surrounding a hair. Magnified 500 diameters by Powell and Lealand's immersion $\frac{1}{8}$th. Negative No. 367, New Series. From preparation No. 3640, Microscopical Section. The preparation was made by Dr. E. M. Schaeffer. The section was stained with carmine, and mounted in Canada balsam. The lobules of the sebaceous glands have been focussed upon for the display of the glandular epithelium; the hair and adjacent portion of the hair follicle appear of course somewhat out of focus.

No. 4. Section of the kidney of a frog. Magnified 400 diameters by Powell and Lealand's immersion $\frac{1}{8}$th. Negative No. 373, New Series. From preparation No. 3018, Microscopical Section. The preparation was made by Dr. J. C. W. Kennon. The section was stained in carmine, and mounted in Canada balsam. The photograph shows some of the *tubuli uriniferi* cut transversely; others more or less obliquely.

No. 5. Section of the liver of a pseudo-triton. Magnified 500 diameters by Powell and Lealand's immersion $\frac{1}{8}$th. Negative No. 371, New Series. From preparation No. 3573, Microscopical Section. The preparation was made by Dr. J. C. W. Kennon. The section was stained in carmine, and mounted in Canada balsam. The photograph shows the polygonal cells and their nuclei. The double contours between some of the cells indicate the position of the ultimate gall ducts.

No. 6. Section of the ovary of a cat. Magnified 400 diameters by Powell and Lealand's immersion $\frac{1}{8}$th. Negative No. 292, New Series. From preparation No. 3031, Microscopical Section. The preparation was made by Dr. J. C. W. Kennon. The section was stained with carmine, and mounted in Canada balsam. The photograph shows the connective tissue of the ovary, with a number of immature ovules imbedded. The nuclei of the connective tissue are well defined, and in several of the ovules the germinal vesicle is distinctly seen.

No. 7. Another portion of the section shown in No. 6. Same power. Negative No. 293, New Series. The photograph shows in the centre an ovule, with germinal vesicle and germinal spot well defined. The ovule is surrounded by several layers of cells, the nuclei of which are best seen on the right side. These belong to the immature zona granulosa. Externally is the connective tissue of the ovary.

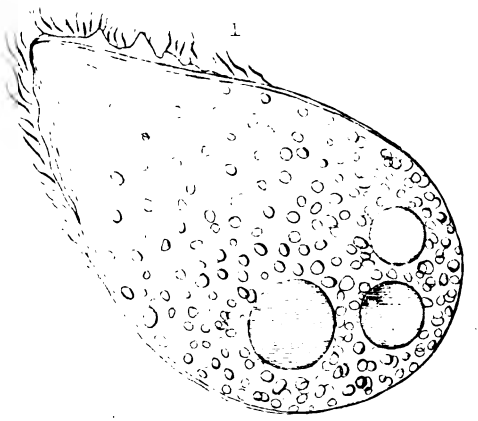
No. 8. Another portion of the same section. Same power. Negative No. 294, New Series. This photograph represents a nearly ripe Graafian follicle, lined by a layer of cells (the zona granulosa) four to six deep—the nuclei of this layer are well defined. On one side an ovule is imbedded, in which the germinal vesicle and germinal spot are distinctly shown. The portion of the zona

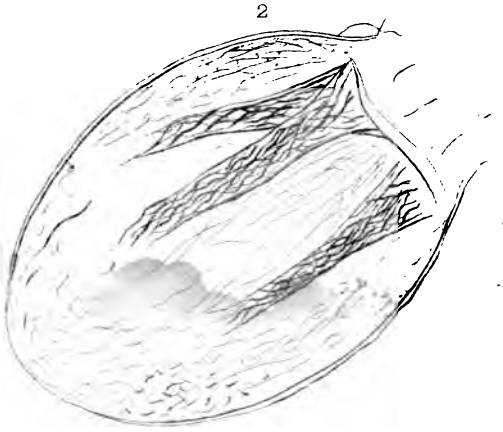
granulosa reflected over the ovule is composed of two layers of cells, as shown by the nuclei. On the exterior of the Graafian follicle the connective tissue of the ovary and its nuclei can be seen. In the cavity of the Graafian follicle is a quantity of ill-defined granular matter.

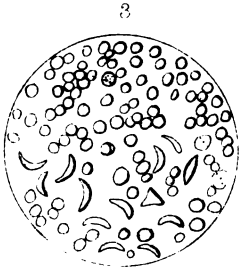
No. 9. An epithelial cell and several salivary corpuscles from fresh human saliva. Magnified 960 diameters by Powell and Lealand's immersion $\frac{1}{16}$th. Negative No. 408, New Series. One of the salivary corpuscles is very satisfactorily displayed.

In conclusion, I cannot but express the hope that my work in this direction may induce other microscopists, and especially those who are conducting original researches, to resort to photography as a means of bringing their results in a tangible form before their fellow microscopists. It will be seen from the statements now made, that, with the exception of the mounting of an ordinary solar mirror, no special apparatus is absolutely needed for this purpose which may not be made by the microscopist himself, or at least produced at a very trifling cost. Nothing more than a knowledge of the principles of photography is required. In every case the services of a good dark-room man should be procured, and if the preparations are carefully selected beforehand and the highest wages paid to the best operator attainable, the average cost of the negatives will be far less than that of the cheapest and most indifferent drawings. Accuracy of representation in these objects can only be satisfied by photography, and photography is not only the most accurate, but the cheapest and least troublesome method of reproducing microscopic objects. Of course much of the beauty of the result will depend on the character of the preparation selected. But in a general way the preparations best fitted for study are those best fitted for photography. If photographic representation was universally demanded, a higher class of preparations than those which now satisfy too many microscopists would become indispensable, and the vague description, based on clumsy or imperfect preparations, which too often disfigures microscopic literature, would be replaced by a more accurate representation of the actual facts.

Army Medical Museum, Microscopical Section.







W. West & Co. imp.

Hæmatozoa from the blood of the "Mantjeh."

III.—*Hæmatozoa in Blood of Ceylon Deer.*

By Boyd Moss, M.D.

Plate XCIX.

About two years since, while examining the blood of various animals, I had one day a specimen of that of a Ceylon red deer (the Muntjac of India) under the microscope. To my great astonishment I saw vigorously swimming across the field several oval ciliated bodies, such as I have represented in Fig. 1 in the accompanying Plate. The front pointed half of the body was covered with cilia in active motion, and as it swam forward through the mass of red blood corpuscles, it was impossible to avoid comparing it to a steam vessel forcing its way through a crowd of small boats. The specimen of blood had been taken about three-quarters of an hour after the death of the animal, while it was still warm—from a wound made by a bullet which had passed through the liver, and I thought it possible that the entozoa came from that organ, but on a careful examination of its substance none of them were found. I had no further opportunity of looking for their bodies for ten or eleven months, when I procured another red deer. About half an hour after death I placed a drop of the warm blood with which the cavity of the abdomen was filled (in opening the animal), under the microscope, and there again was the same curious sight of these entozoa swimming actively across the field. There were generally two or three to be seen at once when using Smith and Beck's $\frac{1}{8}$th objective. They lived under the thin glass cover for about an hour. I now carefully removed the heart of the deer, closing the aorta, as I cut it, with my fingers, and then squeezed out a drop of blood from the ventricle on to a slide; this again presented the same appearance, proving that the entozoa did not come from the cavity of the abdomen. A month or two later I examined the blood of another Muntjac, with similar results. Now, these bodies are of course far too large to pass through the capillaries, and must evidently inhabit the heart and larger vessels, and I had hoped before this to have been able more thoroughly to investigate the matter, and therefore delayed sending this communication to the 'Microscopical Journal,' but I have as yet had no further chances, and the above must be taken *quantum valeat* for the present. The appearance Fig. 2 in the Plate is that presented in all cases by the Hæmatozoa about twenty-four hours after death. When the drop of blood still remained partly fluid beneath the glass cover, they showed three curious bands, which much resemble muscular fibre, but which are not visible during life. They (the bodies themselves) have no colour, being perfectly translucent; a distinct double membrane is to be seen round them. They all

have two or three large spherical bodies like ova towards the posterior half, the remaining portion being filled up with small cells and granules. The cilia are raised on a substructure of a cave-like appearance. The red blood corpuscles of the Muntjac agree in size with those of the musk deer, being only about $\frac{1}{6500}$ inch in diameter, many of them on being placed on the glass slide assume the curious semilunar form represented in the Plate (Fig. 3). I have once or twice examined the blood of the Sambur deer for these bodies, but have seen none.

CEYLON, *May*, 1871.

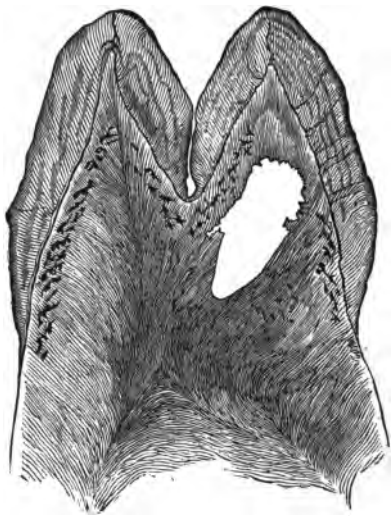
With reference to my communication in your Journal for December last, I have shown specimens to Mr. Thwaites, of the Botanical Gardens in Kandy, and it is his opinion that the organisms can be nothing but spores of some description of fungi. I am quite certain that they existed imbedded in the muscular fibre, and were not accidentally derived from without, and in the Plate facing page 48 of your Journal for February, in the present year, there are some drawings of spores (Figs. 2, 3, 4, 5, 6, 9, 10, 14, 22, and 24), the very counterparts of which I can show in specimens of muscular tissue.

IV.—*Microscopical Fissures in the Masticating Surface of Molars and Bicuspids.* By J. H. M'QUILLEN, M.D., D.D.S., Professor of Physiology in Philadelphia Dental College.

IN a recent communication to an American periodical attention was directed to the fact that the minute openings or fissures found in the grinding approximal, buccal, palatine, and lingual surfaces of molars and bicuspids frequently lead to cavities of some size. Through the kindness of my friend, Dr. R. W. Varney, of New York, who placed in my hands some time since a number of microscopical preparations, I have an opportunity of demonstrating in the most conclusive manner the necessity of immediate attention to such cases.

In the accompanying illustration (which I had made of a longitudinal section of an inferior molar, as seen under an $\frac{8}{10}$ objective and No. 1 eye-piece, magnifying sixty diameters), it will be observed that a minute fissure, invisible to the naked eye in the section, extends from the bottom of the sulcus on the grinding surface of the tooth, through the enamel, almost to the dentine, and enlarging at the lower part into an oval cavity. This is entirely the result of defective formation, the enamel prisms having failed to coalesce at that point, and thus a condition is presented favourable

to the retention of fluids and semi-solids, which undergoing decomposition would speedily destroy the thin septum of enamel covering the dentine. In the latter tissue, closely contiguous to the enamel, a number of black spaces (the *interglobular spaces*) will be seen. Here again is located defective structure and a prolific predisposing cause of decay. The large space represents a carious cavity commencing on the approximal side of the tooth.



In a paper read before the American Dental Association at the meeting held in Boston, August, 1866,* giving the results of a personal examination of the *interglobular spaces*, I remarked, "As evidence of the practical bearings of these investigations, it may be well to direct attention to the fact that the existence of the spaces in teeth which have completed their growth must be regarded as an *abnormal condition*, predisposing such teeth to decay, and that when either by mechanical action, as by a fall or blow, or by the penetration of external caries, such spaces are reached, the *disease here would run riot;* hence the importance of care on the part of patients and operators to have the most minute cavities filled; for though reached only through a microscopical opening, the result would be the same, while, if protected from the action of external influences or the *exciting causes of decay*, this *predisposition* might remain dormant for a lifetime, as is sometimes the case with other diseases."

With no disposition to revive a useless discussion, or to dwell upon the very unreasonable, not to say absurd, objections and denials which were offered to the communication by doubtless well-meaning but mistaken men, at the same time I cannot but regret that they influenced the opinions of others who, reposing implicit faith in their acuteness and judgment, very naturally regarded the objections as of a valid character, until an opportunity for observation convinced them to the contrary. Such was the case with the gentleman who handed to me the specimen under consideration.

It is to be hoped that the illustration offered will teach a valuable lesson to those who have been in the habit of dismissing

* 'Dental Cosmos,' vol. viii., p. 113.

their patients with the statement that "there are some small cavities in the teeth which can be left without disadvantage until another time." It also shows most clearly the necessity of following up the fissures, which generally extend from a central cavity of decay in the grinding surfaces of molars; careless operators contenting themselves with only removing the caries from the central cavity, leave these fissures untouched, and, as a consequence, decay progresses unobstructed and unnoticed, until the tooth is rendered a mere shell.

V.—*Transmutation of Form in certain Protozoa.*

By Metcalfe Johnson, M.R.C.S.E., Lancaster.

Plate C.

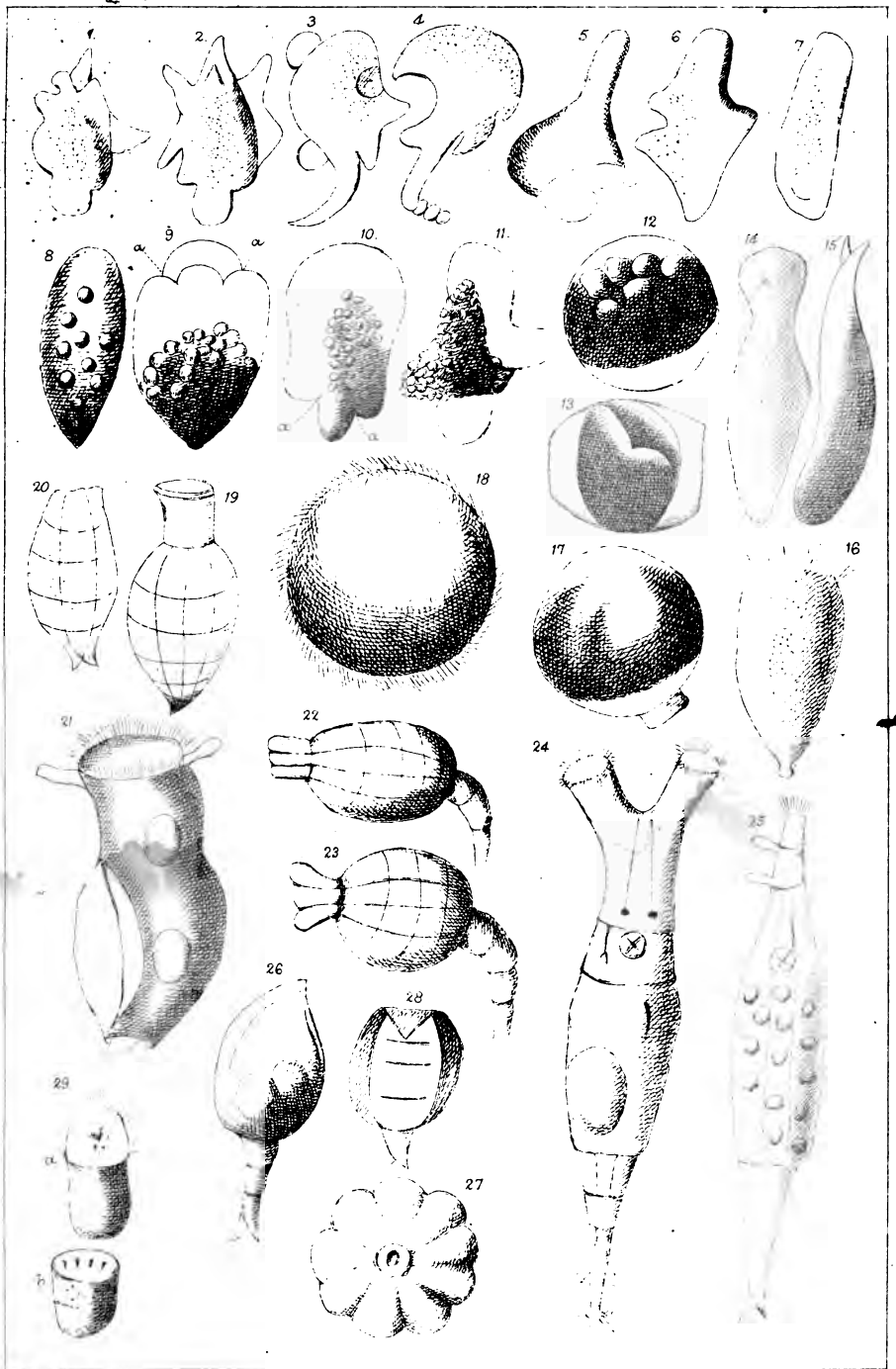
Part II.

On Thursday, August 3rd, 1871, Professor Huxley congratulated the British Association at Edinburgh on the fact that two gentlemen of high attainments, Prof. Thompson and Sir William Thompson, had declared themselves in favour of the doctrine of evolution. It cannot therefore be less a matter of satisfaction to the humbler labourers in this department (the working bees in the hive), that such eminent minds give their support to doctrines which it is the object of their (the working bees) comparatively insignificant researches to establish.

While the *savans* are attending the great annual scientific carnival, held this year in "Auld Reekie," I shall content myself with sitting at home and writing to the 'Monthly Microscopical Journal' on this (to me) important subject of thought and inquiry.

I had proposed to myself, in this next report to the Journal, to trace the origin of some of the more important members of the group of Infusoria to the simple elements known as Monas and its congeners, but owing to the kind sympathy of Mr. G. F. Chantrell, of St. James's Mount, Liverpool, I am induced to devote this paper to the tracing in greater detail the bearing of Paramœcium to Callidina and its allies, the Philodinæa, trusting to reserve the other subject to a future communication.

In a recent letter, Mr. Chantrell writes as follows:—"I have thought a good deal of your papers, and am particularly interested in your subject. I think (on) the first meeting of our private Microscopical Society (the Liverpool Natural History and Microscopical Society) after your last paper appeared, I exhibited a *Carchesium polypinum*, in which the evening before I had noticed some of the Vorticella heads had disappeared, and in their place, amongst the branches, I noticed a globular-shaped substance. On this evening, to my amazement, all the Vorticella heads were gone, and



Metamorphosis of Infusoria

four more globe-forms added, all clustered together. One of our members thought he observed a little ciliary action on one, and when I looked at it again, I saw that it was a Trachelius, and similar to your sketch 63, but minus the ciliary marks. The snout was there, and full of revolving vesicles. It quietly stretched itself out and swam away, and after short intervals all the others made off. Since then I have had an instance of a Trachelius rubbing itself amongst the branches of a Carchesium, and a few of the heads gone and other heads becoming encysted and assuming a pyriform shape; but unfortunately my tank gave way, and there was an end of my experiences. What I have observed in my trough is strongly confirmatory of your observations. I have sometimes had nothing but Amœba; at another time *Paramœcium, Kerona M. Vorticella* in abundance; at another Philodina, *Rotifer vulgaris*, and sometimes Floscularia, &c."

It is in the highest degree satisfactory to find that observers of the careful class to which this gentleman belongs not only sympathize in the researches to which I have now devoted some little time, but are enabled to confirm my suggestions by kindred observations of their own, assisted by their microscopical friends and associates.

In the Plate accompanying this paper (Plate C.) will be found the rough delineation of forms which I have observed in various liquids containing vegetal matter, and which I have watched during their transmutations to forms which have eventually proved themselves to belong to the class Philodinæa. It will be my object to endeavour to show that the members of this class are not forms having specific distinction, such as in tracing their origin we should find derived from separate sources, but simply stages of development of one common form of animalcular existence.

I will first, then, proceed to give some explanation of Plate C.

Figs. 1 to 7 are the varying forms of an Amœba, which, commencing as an *Actinophrys Sol*, passed through its stages, terminating in a form which closely resembles a Paramœcium.

Figs. 8, 9, 10, and 11 are varying forms of a species of Paramœcium, possibly *Loxodes Bursaria*. Fig. 8 represents the Infusorium in the natural state, the revolving vesicles being in active motion. Figs. 9, 10, and 11 are under the influence of opium, in which the revolving vesicles are separated from the other part, which is intended to represent globules of sarcode, which in Fig. 11 so closely resembles an Amœba as to render it very interesting in relation to Figs. 1 to 7.

Fig. 12 is a globular condition of a Philodina, showing the vesicles well developed.

Fig. 13 is a more advanced stage of the same Infusorium or Rotifer.

Fig. 14 is an interesting form, as exhibiting a stage of progress towards Philodina.

Fig. 16 is a Paramœcium, or Loxodes, in which the rudiment of the tail-foot is very evident by reflexion of light, while

Fig. 15 shows the tail-foot more fully developed.

Fig. 17 is a globular form of Philodina, with the telescopic tail just making its appearance. All these forms I have repeatedly verified as transitional, having seen them evolve into Callidina, Philodina, Rotifer, or in the other direction to Amœba, &c.

Fig. 18 is simply inserted as a beautiful globular Infusorium, whose whole surface is covered with cilia, which work in regular order, so as to revolve itself slowly and gracefully over the field. It is here magnified, as are all the other specimens, by 250 to 300 ×.*

Fig. 20 is an elementary, probably early, stage of Philodina, the tail-foot being very distinct.

Fig. 19 is a still more developed form of the same Infusorium.

Fig. 21 is possibly *Enteroplea hydatina*, found among a number of Philodinæa in water containing Valisneria, sent from Liverpool by Mr. Chantrell.

Figs. 22 and 23 are forms of Philodina modified by opium.

Figs. 24 and 25 are the fully-developed forms which I am accustomed to find in water containing vegetable growths. I shall have to refer to them at greater length when speaking of the distinction without a difference which seems to me to have been made by classifiers.

Fig. 26 is another form of Philodina which I frequently find the Callidina assuming in its changes.

Fig. 27 is a perspective view of a Callidina, with the proboscis in the centre.

Fig. 28 is another form, intermediate between Carchesium and Callidina.

Figs. 29*a* and *b* are some rudimentary forms of Infusorium, possibly passing to Floscularia in water containing *Sphagnum palustre*. I have no further history of this Infusorium.

Before proceeding to the general remarks, I will offer a few observations on the subject of the Plate.

In the first place, it has long been my opinion that *Actinophrys Sol* is only a phase of the life-history of Amœba.

It will be observed that Mr. Chantrell speaks of the form of Infusorium found in his researches, as varying at different times between Amœba and other forms. I have often referred to the change from Globular to Amœba.†

* If any of your readers will kindly name it for me, either by letter or through 'M. M. J.,' I shall feel obliged.

† See 'M. M. J.,' Aug., 1869, p. 104, line 2; May, 1871, p. 255, lines 16, 17, and 18.

April 22, 1871, I found *Actinophrys Sol* and Amœba in the same water in which *Sphagnum palustre* had been put.

The observation recorded in the Plate (Figs. 1 to 7) was made July 20th, 1871, and is interesting and important as having been made, not by myself, but by a perfectly disinterested person, a friend of mine, Mr. F. Housmann, who is a very accurate draughtsman, but who, in consequence of his being deaf and dumb, and not a microscopist, is not in a position to have any preconceived views, and was not aware of what my own were upon the subject. I simply observed the *Actinophrys Sol* (from water sent me from Liverpool containing Valisneria), and its first change to an Amœba. I then left my friend to make drawings of any changes he might see. On my return home, he presented me with the piece of paper which I herewith enclose,* on which were the drawings of changes in the Amœba until the water dried up.

The information derived has two points of interest; the first, the probable identity of Actinophrys with Amœba; and second, the association of Paramœcium with the other two forms of Infusorium.

In Fig. 9 (which I watched passing to its present state of protruded sarcode from a form similar to Fig. 8) will be observed the two depressions *a a*, which give a resemblance to forms 52 and 53 in Plate LXXXV., May 1871, and which I am accustomed to recognize as decidedly transitional forms of Callidina.

In Fig. 10 *a a*, similar depressions give rise to a central tongue-like process in the middle.

The white portion represents the protruded sarcode, which in Fig. 11 presents such a close resemblance to an Amœba.

If we compare Fig. 14 with some of the figures in the former Plate, LXXXV., we shall see reason to consider it a transitional form, especially when associated with Fig. 16, which shows the white spot which reflects the light, and is in all probability a rudimentary tail-foot, especially when viewed in connection with Fig. 15, in which it is more marked.

Figs. 14 and 15 were drawn at the same sitting, March 29, 1871, from the same drop of water, which I had prepared by inserting a handful of dead leaves, in December, 1870, into about a pint of water in a glass beaker, in the light and air of my study.

Figs. 19 and 20 are forms which I recognize daily as progressive towards the more mature forms shown in 24 and 25.

Fig. 21 is a developed form, which I do not doubt is traceable in its origin to the same source as the rest.

In Figs. 22 and 23 we have a strong resemblance in form to Fig. 50, Plate LXXXV., which presented a very interesting phase of change as a Vorticella.

* Mr. Johnson sent us the piece of paper which contained drawings *exactly* similar to those in the accompanying Plate.—Ed. 'M. M. J.'

Fig. 26 is another similar change, which I have often seen from decided Callidina or Philodina.

Fig. 27 shows the profile view, which as it turns again in the drop of water reveals the perfect animalcule in its most active form.

It would seem here as if there were much reiteration, but in a position such as mine I feel as if every step requires the utmost detail in its construction, lest I should suddenly find myself over head and ears in a quicksand, with only a mare's nest to reward the venture; but if, as it seems to me, there is firm and solid truth near at hand, the journey to it is worth the attempt.

It is now my duty to undertake a part of the question which is more than usually difficult, *viz.* to show that the great system of classification is on an uncertain basis, and I will take the family of Philodinæa as an example, and I think the Figs. 24 and 25 may assist in showing my point.

The family is divided by Griffith and Henfrey, in the 'Micrographical Dictionary,' into *Callidina*, *Hydrias*, *Typhlina*, *Rotifer*, *Actinurus*, *Monolabis*, and *Philodina*. The characters of some are sufficiently doubtful to admit of removal at once. Thus Griffith and Henfrey say, p. 358, of *Hydrias cornigera*, pl. 34, fig. 39, "probably a young and imperfectly examined Philodina," and of Typhlina, "an imperfectly examined genus of Rotatoria, of the family of Philodinæa;" and p. 463, of Monolabis, "Eyes two, frontal; tail-like foot with two toes; horns absent."

Dismissing the former two, Hydrias and Typhlina, as "imperfectly examined," we come to Monolabis. Now it is evident to all observers of these Infusoria, that the perfectly formed animalcules, such as Fig. 24, are very frequently observed to present these marks, tail-like toes two, horns absent, simply because the creature does not remain long enough under inspection.

I have frequently seen Fig. 24 in such a condition, and the next minute protrude another joint of the tail and expose the two lateral horns above the foot as well as those in the cervical region. I am therefore of opinion that these characters in animalcules of such changeable form are not sufficient to warrant a separation into a fresh class. Let us therefore temporarily dismiss this division, and look at the remaining four.

Callidina, then, is distinct from Philodina, by the rotatory organ not being furnished with a stalk; but in the two Figs. 24 and 25 (which I have repeatedly seen transmutable) we have no characters by which to divide them. Here we have eyes absent often, because we have not that part sufficiently developed, as in Figs. 25, 22, 23, or in Figs. 19 and 20. But I cannot persuade myself that these points, either absent or present, visible or invisible, at any one observation, are any other indication than the stage at which the animalcule has arrived at the moment of inspection.

Rotifer is described (p. 603) as having eyes on the proboscis, foot with two terminal toes, and lateral horn-like processes.

These characters, again, I say are so variable, according to the stage of development, or even temporary elongation, as to be valueless as points of distinction.

Actinurus (Griffith and Henfrey, p. 14) "agrees with Rotifer in general structure." The point of difference being three and five points to the toes. But this question appears to me so entirely to depend on stage of development and state of projection as to be entirely unreliable as a distinctive mark.

So far as my few observations have enabled me to form an opinion, I consider that the facts give colour to the opinion that the objects figured in the accompanying Plate (Plate C.) are not only stages of development of one and the same Infusorium, but that these and several other forms are merely developed states of some earlier and more simple forms, which I trust soon to be able to call attention to.

The subject in all its bearings is a very large one, and has so close a relation to the Darwinian hypothesis, as well as so important a bearing upon the various schemes of classification, that one is inclined to hope that some sympathy is felt amongst certain classes of microscopists who, like Mr. Chantrell, are able and willing to bear testimony for or against the opinion. Facts "against" are as welcome as facts "for," where truth is the object of search. I trust I shall not be exceeding my privileges in addressing the 'Monthly Microscopical Journal,' if I here take the liberty to say, that if among the readers of the Journal there are those whose pleasure has led them to investigate this question, and whose leisure will permit of their writing upon the subject, if they will, either through the pages of this valuable channel of intercommunication, or by private post, express their opinion either for or against these views, it would confer a favour on all who are solicitous for the publication of truth. It is, I am quite aware, in so difficult a subject as the education of the eye in microscopy, and with the temptations of prejudice to lead one astray, a most easy and probable result of inquiry that it should end in error. But since "ex nihilo nihil fit," and "nothing venture nothing have," are proverbs which have stood the test of experience, I trust I shall be held excusable for having cast loose the anchor of faith in old systems, and venturing my little boat upon the great sea of speculation and inquiry. There is one writer, whose name is unknown to me, but who is the author of a paper in the 'Quarterly Review' under the title of "Higher and Lower Animals," whose opinion upon classification seems to me so very near the truth, that if this venture should meet his eye it will be esteemed a favour if he would kindly permit me to hear from him, either under a "nom de plume" or otherwise.

My own view of the great question involves what I venture to term the Unity of Nature, that is to say, that so far as our senses will at present carry us, we see no separation of nature's works into classes, except in a very broad and rough investigation; and whatever class we examine, whether of the animal or vegetal kingdom, there are always some members whose characters approach so closely to those which are considered as indicative of another class, that we are perpetually brought face to face with objects which it is impossible to affix the distinct section to which they properly belong. Thus, whether we divide nature into animal, vegetal, and mineral—into organic and inorganic, or the animal world into vertebrate and invertebrate, or the vegetal into endogen, exogen, and cryptogam, or whatever way we choose to divide it, in no case can the boundary line be so drawn as to decide definitely where to place every specimen of nature's products. This is due to the limits which are drawn around our senses, and in that section of nature to which our attention is at present directed the highest powers of the microscope are unable to reveal to us the smallest of nature's organisms which develop the phenomena of life. Testimony is borne by Dr. Lionel Beale to this effect, and he is understood to make use of the highest powers that have at present been produced. But independent of this extreme minuteness of particles of matter, we have reason to consider it as certain as anything we may be said to know, that there is a definite point at which the unit condition of matter commences its existence, and within the limits of these two considerations we seem to arrive at a view of life consistent with law, with harmony, and with what we call "creative wisdom."

This view, as supported by the principal *savans* at the Edinburgh Congress, is in accordance with Harvey's law, "Omne vivum ab ovo."

In the consideration of the objects to which our attention is directed in the present remarks, we are led only to the contemplation of them as passing through various stages of growth, and possibly in some cases of transmutation somewhat allied to those imago changes of which the insect class furnish us with such beautiful and instructive examples. In dealing with the further details of the progress of growth from less developed forms we shall have occasion to notice that there is considerable evidence to show that the form in which the Infusoria commence life as independent organisms is not always the same, for there is some reason to believe that considerable development may take place within the parental cavity before the proles is extruded for independent growth. Although this subject must be deferred for more mature consideration in another paper, yet it may help us to contemplate some of the forms which we have now before us with

greater correctness if we bear in mind that this may take place. Thus the Euglena, which possesses some of the properties of change of form which are homologous of those here witnessed, is evidently capable of arriving at the condition of green bodies containing chlorophyll masses with the red eye before it is extruded from the parent organism, while it is also capable of arriving at a similar stage of existence through several conditions, as proved by my early experiments detailed in my first communication to this Journal.*

The present view of the Infusoria has two points of importance; in the first place, if accepted, it reduces the classification to a simpler and less confusing state, and in the second, it offers a self-evident explanation of their constant appearance under all circumstances in which atmospheric air is a factor of the accidents of life. The first of these points is one of importance, as may be shown by reference to any book of scientific classification and nomenclature, from which it will be seen that the same object is often indicated by different names given by the early or original observer, and in the case of the Infusoria, since for so long a time Ehrenberg was their principal if not sole expositor, it has become matter of course that where this great naturalist had in the first instance given a name and a class which subsequent investigators have found it necessary to modify, much confusion has arisen from one and the same object being spoken of and classified under two different plans. Thus *Loxodes Bursaria* is called by another observer *Paramœcium Bursaria*, Pleuronema is called Paramœcium, and many others in similar manner.

I trust that I have now shown some cause for believing that transmutation of form applies very freely to this class of nature's objects, and that we shall thus the more readily enter upon the consideration of the origin of the Infusoria generally in Monas and its congeners

LANCASTER, *August*, 1871.

Many plants are known by two or three names, according to the observer, *e. g. Gongosira clavata* (Kützing) is *Conferva multicapsularis* (Dilwyn), *Protococcus viridis* is Chlamidomonas of Ehrenberg and Diselmis of Dujardin: Protococcus and Chlorococcus are also liable to much confusion.

* See 'M. M. J.,' Aug., 1869, p. 101, line 41.

VI.—*On Gnats' Scales.* By Jabez Hogg, Esq., Hon. Sec. R.M.S.

It is said that history repeats itself decennially: if this be a truth in the progress of nations, it appears to be scarcely less true with regard to science, which not unfrequently furnishes to the pages of our periodical literature an old discovery furbished up in so attractive a dress that it sometimes passes as a newly-recorded fact. This is certainly the case with a supposed discovery referred to at page 102 of the August number of the 'Microscopical Journal.' The writer mentions that "in the spring of 1868, while engaged in examining the head of a gnat, he was surprised to discover that the proboscis and palpi were clothed with scales entirely like those of butterflies." The paper on this subject appears in a German scientific journal, "under the joint authorship of Dr. E. Muller and Professor F. Delpino," and it has been translated for the 'American Naturalist' by Mr. R. L. Packard, a gentleman well acquainted with the Natural History world, who therefore should have known that there was nothing novel in placing on record observations which appeared in my book on the Microscope published in 1854, and repeated in all subsequent editions, some 60,000 copies of which have made their way to every part of the civilized world. A drawing of the proboscis, clothed with scales, is given page 287 *first edition*, 599 of the sixth edition; a single scale detached is seen near it; and again, at page 611, another scale, more highly magnified, and exhibiting the "*'wavy appearance*" spoken of by the German authors, but which curiously enough does not quite accurately represent the structural characteristics of the scale. The waviness is owing to the under surface being seen rather out of focus. I stated, however, that "the curiously-formed proboscis is covered with feathers or scales;" and in the woodcut a scale is placed side by side with other test-scales from Lepidoptera and Spring-tails, in order that their structural similarity might be noticed. It is not perhaps so very surprising, after all, that this fact should have escaped the observation of German authors, since it has received so little recognition from English writers on either entomology or microscopy. So far as I have been able to ascertain, no one has described this peculiarity of

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DESCRIPTION OF PLATE CI.

Fig. 1.—Battledore scale from proboscis of *Culex pipiens*.
„ 2.—Flattened out scales from body.
„ 3.—Pointed scale from margin of wing.
„ 4.—Trumpet scale from thorax, each × 350 diameters.
„ 5.—Portion of leg and foot.
„ 6.—Clothed proboscis.
„ 7.—Arrangement of scales on wing.
„ 8.—Scale, *Culex* ——, variety unknown.

W.N. Roberts, Aug.st 1871. W. West &

the proboscis of the Gnat; and indeed no one would expect to find so curious a departure from the ordinary characteristic of the order Diptera as that presented by the Culicidæ. Francis Walker, F.L.S., mentions in his 'Insecta Britannica,' 1856, that the veins of the wings of Culex, Anopheles, Corethra, Mochlongx, and Aëdes, "are fringed with scales," but he omits to notice their occurrence on other parts of the insects. As both German authors and American translator have repeated my error of the "wavy appearance" of the scale, and made sundry other slight mistakes, it may be as well to enter a little more fully than I have hitherto done into the structure and variety of "Gnats' scales."

Scales are a generic characteristic of the Culicidæ, and although presenting pretty much the same appearance differ essentially in form. There are four distinct kinds: the proboscis, palpi, and legs are entirely covered over with the battledore scales, represented at Fig. 1. The nervures or venations of the wings, and portions of the body of the insect, have regularly arranged rows of scales, shown at Fig. 2; while from the marginal edges of the wings project slender scales, which terminate in a point, Fig. 3. The intermediate portions of the wings and body are covered with fine hairs, the thorax with tufts of feathery scales somewhat peculiar in form, Fig. 4, the pedicles of which are considerably longer than the rest, while the upper part gradually widens out and terminates abruptly in a crenated edge. These may be briefly described as *trumpet-shaped* scales. In each case the scale is inserted by a narrow pedicle into the chitinous membrane, gradually assumes a scutiform appearance, and terminates in a crenate or pointed edge. The basement membrane is homogeneous, and the upper layer is corrugated or traversed by longitudinal ribs, and these again are regularly and finely striated throughout in the horizontal plane. It is the striations on the ribs which, when seen slightly out of focus, give to the scales a wavy appearance.

It is curious to find portions of *C. pipiens* completely clothed, and other parts free from scales. The legs are covered throughout, as shown in Fig. 5, and the feathers seem to take the place of hairs in other families of the Diptera. The proboscis, which is half the length of the male insect, is used as a prehensile organ and as a sheath or scabbard to the exquisitely-formed set of penetrating instruments. A slender needle-like piece, seen in the drawing, Fig. 6, just projecting out of the sheath, is serrated towards the tip, and thickened at the back like a scythe. It is said to be employed as a suctorial apparatus, or, when the insect is irritated, for conveying a fluid which is thought to be poisonous, from the minute receptacle situated at the base of the proboscis. The feathered sheath can, by a little careful dissection, be drawn off the slender set of instruments. To secure perfectly good scales, and preserve them in an

undisturbed state, the Gnat must be secured as it is seen issuing from the larva case. The scales should also be mounted dry, as when immersed in balsam or fluid they become too transparent. They are an excellent test for a $\frac{1}{4}$-inch objective.

It is not improbable that the scales of even the various species of Culex will, after a more careful examination than I have been able to make, be found to differ, as upon going over the collection of Gnats in the British Museum, I discovered the scale represented at Fig. 8. This form closely resembles the scales of Lepidoptera.

The body of *C. annulatus* is entirely covered with alternate rings of dark brown and white-coloured battledore scales; and the hairs projecting from its sides are longer and more numerous than in *C. pipiens* or *C. musquito*. The "feathered antlers" pectinate antennæ of the male insects, although destitute of scales, are exceedingly handsome objects: they surmount the most brilliant set of compound ocelli it is possible to find. In short, he wears finer clothes and is better dressed throughout than his female companion. When the piercing apparatus is sent into the flesh of a victim, the proboscis appears to divide, it is then thrown up and turned back upon the head like the trunk of an elephant.

The Gnat-like midges, so common in this country, and which belong to the Tipulidæ, very closely resemble Culicidæ. *Chironomus plumosus*, often mistaken for a Gnat, has plumed antennæ; but on no part of wings or body can scales be found; the same remark applies to the rest of the species. The resemblance between many of the perfect insects belonging to the genus is striking, while their larvæ differ very considerably from each other. The club-shaped balancers, halteres, are more developed in Tipula than in Culex. The genus *Psychoda* have been thought to be clothed with scales, however they are not; their wings and bodies, on the contrary, are thickly covered with long, peculiar hairs. Some of these little creatures present under the microscope a gorgeous appearance. The various parasites infesting Diptera deserve the attention of microscopists.

VII.—*The Examination of Nobert's Nineteenth Band.*

By F. A. P. Barnard, of Columbia College, New York.

A paragraph in the article "On the Use of the Nobert's Plate," by my friend Colonel J. J. Woodward, of Washington, published in the July number of the Journal, which has just reached me, deserves some notice. The paragraph is upon page 27 of the Journal, and relates to a plan employed for settling the question whether the lines seen by me in the nineteenth band of Nobert's plate, in a series of observations made some years ago, were true or

spurious. The personal question involved is of no consequence whatever; but the trustworthiness of the method employed is another matter; and this, I think, is not at all invalidated by the argument of Colonel Woodward.

This method may be briefly explained thus:—In ruling his nineteen-band plate, Mr. Nobert has *designed* to make the spaces between the lines of the ninth band, measured from centre to centre, just $\frac{1}{5000}$th part of a Paris line each; and those of the nineteenth band $\frac{1}{10000}$th each. When I found, by micrometer, that twenty spaces on the nineteenth band, or any other carefully counted number, had the same aggregate value as half as many spaces on the ninth (where the counting is easy), I considered myself justified in concluding that the nineteenth band was truly resolved; and did *not* think it necessary, in order to prove this, that I should actually count the band clear across.

Colonel Woodward, however, says that this method will not do, because the spaces have not probably the values nor the relations intended by Nobert, and stated by him to exist. That the *absolute* values of the divisions are not exactly what was designed is presumable enough; because it is not possible to construct a scale or anything else in so strict accordance with the intention as to be without error altogether; but in respect to the *relations* between the spaces of two scales ruled by the same machine, there is hardly room for the same degree of uncertainty. The parts of a ruling machine by means of which the distances between the lines ruled are regulated, are not microscopic. They are so large that any minute irregularity in them would bear no appreciable ratio to their total magnitude. Moreover, when we consider that the same part of the machine—a ratchet, for instance—bears the divisions by which both the larger and the smaller spaces are regulated, so that—to take the case before us—the very same instrumental measure determines the *aggregate* value of ten spaces of the ninth band, and of twenty spaces of the nineteenth, we shall see that there *can* be no sensible error in assuming that the ratio between the mean breadths of these spaces is as two to one. There can be none, at least, except on the supposition of wilful mis-statement by Mr. Nobert—a supposition which his known integrity makes inadmissible, and which is, moreover, abundantly disproved by Colonel Woodward's own photographs and measurements.

Colonel Woodward, however, thinks that the ninth band is broader than the nineteenth; whereas, since the former has but twenty-six spaces, while the latter has fifty-six, the nineteenth should have the greater breadth if the spaces are truly to each other in the ratio of two to one. Supposing the total breadths of the bands equal, the numbers just given enable us to state the real ratio to be as two and two-thirteenths to one; or, as Colonel

Woodward thinks the ninth band really the narrowest, we may be justified in saying that his hypothesis makes this ratio as great as $2\frac{1}{8} : 1$. Is it for a moment conceivable that a ruling engine of any sort—to say nothing of one of such exquisite workmanship as that must be which has turned out these miraculous achievements of Mr. Nobert for so many years—should be capable of making errors in its divisions to the extent of one part in thirteen?

If we suppose, however, the relation of the spaces in these two bands to be such as Mr. Nobert intended, then the ninth band is not the broader of the two, but the nineteenth. The difference would be $\frac{4}{120000}$ths of a line, or $\frac{4}{120000}$ths—say $\frac{1}{30000}$th of an inch. This small space is undoubtedly measurable; but in the present instance it is rather difficult to measure, because, first, the ruled lines themselves are heavier and broader in proportion as the spaces are wider; and because, secondly, when the lines are truly in focus, the false lines on the margin make it often rather doubtful where the band begins.

But now, without making any question about the absolute or the relative values of these spaces, any further than to assume that, in the latter particular, the deviation from the intention is not greater than Colonel Woodward supposes, though I am willing to allow it, if necessary, to be as great as $2\frac{1}{4} : 1$, I think I may reasonably claim that my list was a perfectly trustworthy one for the purpose for which alone it was applied, *i. e.* to settle the question whether the nineteenth band was or was not truly resolved. Anyone who has studied close rulings under the microscope, with varying obliquity of illumination and parallel rays, will have observed two species of illusion so perfect as to deceive the most practised eye, and even his own if he happens not to know the object at which he is looking. One of these presents the number of lines exactly doubled, and the other shows exactly half the true number. The physical cause of these appearances I believe I am able to explain; but the explanation would be here out of place. Whenever either of these illusive appearances presents itself, the lines are perfectly smooth and clear, and continuous from one end of the band to the other. They will inevitably be taken by the unpractised observer for the real lines. But the ordinary appearance of the unresolved bands is not this. The spurious lines which appear before resolution are in general rough, irregular, and, to use a word which best expresses the appearance, *blotchy*. The number of these rough lines which may be seen in a given band is not very easy to count; but when I have succeeded to a certain extent in counting, I have found that it is never with me in a simple ratio of 2 : 1, or of 1 : 2 to the number of real lines known to be present. On the other hand the ratio is as inconstant as possible.

Now for the bearing of this fact of observation upon my test. In looking at the nineteenth band, I saw it as a series of perfectly regular, well-defined, uniformly distant lines, covering its whole surface, and extending unbroken across the entire field of view of my microscope. The question with me was, Do I see the true lines, or is this appearance the illusion in which only half the true number are present? In the latter case, I shall find in a given space the same number of lines which I find in the ninth band in the same space; in the former, I shall find twice as many. Now, if I had *not* found *exactly* twice as many (which I did), I should have still believed the band to be resolved; for it was impossible to suppose that I saw only *half* the number of the true lines; and equally impossible to suppose I saw twice the number of the true lines; therefore, if I had observed (as I did not) any seeming deviation from the ratio of 2 : 1, I should have been compelled to account for it on a hypothesis such as Col. Woodward has suggested, *viz.* that the relations between the spaces in these two bands are not exactly in accordance with the intention of the constructor.

There is a peculiarity of this plate brought to view by this discussion which has never been noticed, though apparently introduced with a purpose. The first band has seven lines, and purports to measure from centre to centre of the bounding lines just $\frac{6}{1000}$ths of a Paris line. The next has ten lines, and similarly measures $\frac{9}{1500}$ths of a Paris line. The third has thirteen, and measures $\frac{12}{2000}$ths. All these fractions, reduced, give the same value $\frac{3}{500}$ths of a Paris line, as the common total breadth of the bands. But the fourth band has fifteen lines only, or fourteen spaces. The spaces Mr. Nobert states to be 2500th parts of a Paris line; so that if there had been sixteen instead of fifteen rulings in the band, the total breadth would have been $\frac{15}{2500}$ths $= \frac{3}{500}$ths as before. On the other hand, if fourteen spaces occupy $\frac{3}{500}$ths of a Paris line, each space must be greater than $\frac{1}{2500}$th. Considering that, with a dividing machine, it would be much easier to follow up the series $\frac{1}{1000}$, $\frac{1}{1500}$, $\frac{1}{2000}$, $\frac{1}{2500}$, than to substitute, in place of this last term, $\frac{3}{7000}$, as the second supposition would require; and considering that this substitution would be a contradiction of Mr. Nobert's express statement of values marked on every one of the plates, we may fairly conclude that the dropping of a space argues no interruption of the law regulating the values of the divisions, but does argue a reduction of the total breadth of the fourth band. The sixth band contains seventeen lines, or sixteen spaces. If the rulings had been nineteen, and the spaces eighteen, the total breadth of the band would have been $\frac{18}{3000}$ths $= \frac{3}{500}$ths as before. In short, if the aim had been to keep the total breadth of the band always the same, there would have been added always three lines in

proceeding from each band to the next. The nineteenth band would in that case have had sixty-one lines and sixty spaces, and its breadth would have been $\frac{60}{10000}$ths or $\frac{3}{500}$ths of a line like all the rest. In point of fact, the number of lines added in passing from band to band is three in only twelve instances, two in five instances, and four in one instance. An apparent law regulates this matter till we approach the more difficult bands. Thus, there are first two *threes*, then two *twos*, then two *threes* again, and then two *twos* once more; but then follow *three, four, three*, and we are now at the twelfth band where the difficulties become serious. My opinion is that Mr. Nobert introduced this irregularity to make it impossible for anyone to state with certainty the number of lines in any of the higher bands without having actually counted them. Had the law of *threes* been followed throughout, it would of course have been easy to ascertain the number of lines in any band by a simple calculation. He did not believe that any band beyond the fourteenth would ever be optically resolved. He stuck at that band for a long time himself, and in its divisions he had reached the limit of visible magnitude assigned by Fraunhöfer. Knowing then himself, and he only knowing, the numbers of lines on these higher bands, and having made it impossible with certainty to calculate them, he had an infallible criterion for determining whether an imagined resolution had been real. Colonel Woodward having met this difficult test, and having counted for him his lines in every band, he has been satisfied, and has frankly admitted that the thing he deemed impossible has been accomplished.

July 19th, 1871.

PROGRESS OF MICROSCOPICAL SCIENCE.

Experiments on Spontaneous Generation.—Mr. F. Crace-Calvert, F.R.S., has been recently making some experiments in this direction which have led him to conclude that spontaneous generation is an impossibility. They are published in the 'Proceedings of the Royal Society,' No. 128. The more interesting were those made relative to the heat which the lower organisms can tolerate. The first series of experiments was made with a sugar solution. Sugar was employed, being a well-defined organic compound free from nitrogen, which can easily be obtained in a state of purity. To carry out the experiments he prepared a series of small tubes made of very thick and well-annealed glass, each tube about four centimètres in length, and having a bore of five millimètres. The fluid to be operated upon was introduced into them, and left exposed to the atmosphere for sufficient length of time for germ-life to be largely developed. Each tube was then hermetically sealed and wrapped in wire gauze, to prevent any accident to the operator in case of the bursting of any of the tubes. They were then placed in an oil bath, and gradually heated to the required temperature, at which they were maintained for half an hour. A solution of sugar was prepared by dissolving 1 part of sugar in 10 parts of water. This solution was made with common water, and exposed all night to the atmosphere, so that life might impregnate it. The fluid was prepared on the 1st of November, 1870, introduced into tubes on the 2nd, and allowed to remain five days. On the 7th of November twelve tubes were kept without being heated, twelve were heated to 200° Fahr., twelve to 300°, and twelve to 430° Fahr. The contents of the tubes were microscopically examined on the 1st of December, twenty-four days after heating, with the following results:—

Sugar Solution not heated.	Heated for half an hour at 212° Fahr.	Heated for half an hour at 300° Fahr.	Heated for half an hour at 400° Fahr.	Heated for half an hour at 500° Fahr.
There were about thirty animalcules under each field of the microscope, principally *small black vibrios*, two or three micrózymes swimming slowly about, three or four *ordinary swimming vibrios*, and a few Bacteria.	A great portion of the life had disappeared; no animalcules were swimming; still this temperature had not completely destroyed life. Four or five *small black vibrios* were observed moving energetically to and fro; two or three *ordinary vibrios* were also observed moving energetically in the same position of the field; that is, without swimming about.	The sugar was slightly charred, but the life was not entirely destroyed, as one or two *ordinary vibrios* and one or two *small black vibrios* were observed in motion under the field of the microscope.	The sugar was almost entirely decomposed; no trace of life was observed.	No life observed.

Recent Foreign Papers.—Professor Stricker's 'New Year-Book of Medicine,' of which two parts have been published, contains a series of valuable papers on microscopy. Among them may be mentioned the following:—There is one by Dr. Hausen on the results of inflammation in the corneal tissue; another by Dr. Güterbock on the effects of inflammation of tendons; by Dr. Yeo on the pathology of inflamed lymphatic glands; by Stricker himself on the nature of the poison of pus, and another paper in conjunction with Dr. Albert—an account of traumatic fever; Dr. Lang gives the pathology of inflammation of the bones, and Dr. Kimdrat a paper on the inflammatory changes in the endothelia of serous membranes.

Researches on Inflammation and Suppuration.—At a meeting of the Royal Irish Academy, held on May 8th, Dr. J. M. Purser read the second part of his report on the above subject. It is reported by the 'British Medical Journal,' which gives a very full account of the paper. Herr Cohnheim believes that the two following propositions are established as the result of his investigations:—1. In an inflamed part, the white corpuscles of the blood pass through the walls of the vessels in great numbers, and, having become free in the tissue, constitute the cells of pus. 2. The cells of the inflamed part itself have no share in the formation of pus; they persist for a time unchanged among the emigrated blood-corpuscles, and, if the inflammation last long enough, or attain a great intensity, they undergo a series of changes of a purely regressive or degenerative nature, ending in their death or destruction. Dr. Purser, in the first part of his report, read twelve months since, stated that his own observations fully bore out Professor Cohnheim's views as enunciated in the first of the above-quoted propositions. So far back as the year 1846, Dr. Augustus Waller had described the passage of the leucocytes of the blood through the walls of the vessels. With regard to the second proposition of the German physiologist, however, Dr. Purser found that the experiments conducted by himself gave negative results, and in them he was borne out by the opinions of Virchow and of Goodsir. Having described Professor Cohnheim's mode of procedure in experimenting on the corneæ and tongues of frogs, Dr. Purser proceeded to give in detail the results which he had himself obtained. His observations were also made on the corneæ and tongues of frogs. Inflammation was excited either by cauterization with nitrate of silver, or by the insertion of a seton. In some instances, the occurrence of a spontaneous ulcerative keratitis obviated the necessity of causing irritation. Phenomena, essentially the same in kind, but varying much in degree and as to the time of their development, showed themselves in every case. On no occasion did the connective-tissue cells remain unaltered among the pus-corpuscles. The first well-marked change observed in the former consisted in a tendency to become elongated, and, in doing so, to lose their equally stellate shape. Their nuclei underwent a similar modification of form, and the protoplasm assumed a more decidedly granular appearance than in health. In the next stage of the inflammatory process, the cells have completely lost their primitive form, and have become perfect spindle-shaped bodies, while the number of nuclei

increased, and amounted sometimes to four or more in a single cell. The third change consisted in the division of the spindle-shaped corpuscles. These first assumed an hour-glass appearance, and finally divided across in one or more than one place. Sometimes the spindles did not divide, but formed movable, multi-nucleated masses, like those described by Stricker. Dr. Purser believed, too, that the researches of this physiologist on inflammation confirm his own observations.

NOTES AND MEMORANDA.

Mr. Wenham and Mr. Tolles.—In reference to Mr. Wenham's last communication, we have received a letter from Mr. Charles Stodder, of Boston, relative to his supposed share in the controversy. He thinks that Mr. Wenham does him "too much honour when he associates" his name with that of Mr. Tolles, in the matter of the angle of light admitted to an objective. He does not presume to have or express any opinion either on one side or the other. It seems that his note, which we published, was intended to be a private note, containing a correction. As, however, it was not marked private, we could not imagine it was intended as such, and hence, for full correction sake, we published it. Mr. Tolles does not wish to have his name mixed up in the discussion. *En passant*, we beg to point out that there was an error in Mr. Tolles' paper, the word *suf*fraction being printed by a stupid mistake for *re*fraction.

CORRESPONDENCE.

Nobert's Nineteenth Band.—Col. Woodward.—Mr. Stodder.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *July* 20, 1871.

Mr. Editor,—I have no longer any controversy with Col. Dr. Woodward on the question whether he or Mr. Greenleaf and myself were the first to resolve the nineteenth band of Nobert's plate. I have said on that, all that I need to say.

But his last paper, in the July number of the 'Monthly Microscopical Journal,' has some propositions that are subjects of fair criticism —some that I dissent from, and must point out, or my silence would be claimed as assenting. Therefore I ask the privilege of submitting my views to the "goodly company" of microscopists, who must decide.

Dr. Woodward opens his case, by saying that he does not think that the question of priority as to the resolution of the nineteenth band possesses sufficient general interest to make it worth while for him to

add anything to what he has already written. What he has written is evidence that he then thought it was a matter of great interest. It is true the question does not, outside of the circle of microscopists, possess the interest that the invasion of France by Prussia did in the political world; but within that circle it does possess as much interest as does the cession of Alsace to Prussia in the political.

The first construction of an instrument capable of that feat marks an event, an era in optics, as important and remarkable as the renowned improvements of Lister and Andrew Ross.*

Dr. W. says, three criteria for distinguishing the spurious from the true lines have been offered. He specifies, "The first is the unaided judgment of the individual microscopist, who is supposed to be able instinctively to distinguish the false from the true lines, without any special help." By whom was such a criterion ever offered? Certainly not by me.

On the same page—in the same breath, so to speak—Dr. W. continues: "It may be granted that an observer who has many times effected the true resolution of any given band, will at length have its appearance so firmly impressed upon his mind that he will recognize it whenever he sees it, as he would the face of a familiar friend; but this familiarity, which all acquire with any appearance which they have many times reproduced, will only serve to mislead, if at the beginning spurious lines have been confounded with the true, for then the deceptive spurious appearance will be sought for as eagerly as though it were the true one." Good! Dr. W. having granted that, has granted all I ask for. That has been my principle of observation, caution and all. I could recognize the true lines as the face of a familiar friend.

A few other passages in Dr. W.'s paper require notice. He says the question is "simply whether the modern objectives as actually made have a field sufficiently flat to resolve from edge to edge a series of lines occupying a space of the 2000th part of an inch wide." No question like that has ever been offered or suggested by anyone before to my knowledge, not even by Dr. Woodward himself. Flatness of field is a desideratum in an objective, but not the *sine quâ non*. The real question is, can an instrument be made of such *defining* power as to separate or to show the lines or the spaces between two lines ruled to the fineness that the nineteenth band is? Mr. Huxley said, "Histologists, he feared, had come to the end of their work unless . . . . they could obtain microscopes which would enable them to separate two points the 100,000th part of an inch apart."† Mr. Huxley can scarcely be posted as to what modern microscopes can do. If the lines of the nineteenth band are as wide as the spaces between them, they are only 1—224,000th of an inch apart.

The problem is strictly analogous to that of separating double stars—a matter of definition. If the microscopist can see the spaces

* See Carpenter, 'The Microscope and its Revelations,' 1st ed., 1856, p. 197, Philadelphia, and every edition since to 1868; also Dr. H. H. Hagen's Remarks, 'Proc. Boston Soc. Nat. Hist.,' 1869, vol. xii., p. 359.

† 'M. M. Journal,' Nov., 1870, p. 291.

between two, three, five, or ten lines of a certain band of the plate he has resolved it. If he doubts the ruling, or wishes to verify he may count and measure until he is satisfied. It was entirely optional with the artist to rule ten lines or ten hundred—the problem of resolution is the same. There is no special virtue in fifty-seven lines. Dr. Woodward's criterion of the resolution is as much a test of the "flatness" of the glass on which the lines are ruled, as of the "flatness" of the field of the objective. It will readily be perceived by anyone who has tried the resolution, that so minute and delicate are the lines, that any curvature of the glass, or variation even in the thickness, will throw some of the lines out of focus.

I readily acknowledge the justness of one of Dr. W.'s criticisms on my original paper. It was an oversight to suggest that the micrometer must be moved the 100,000th of an inch only. That, however, does not remove the difficulty of counting "such fine lines." I never said it was impossible.

I regret exceedingly that Dr. W. should think that I have shown unfairness to him in representing his remarks: it was my endeavour to represent them fairly, and I believe I have represented them as they were understood by all my friends. I was acquainted with Dr. Hagen's assertion which Dr. W. quotes, though I do not read the original I know that Dr. Hagen wrote that none of Tolles' objectives had resolved the nineteenth band, and wrote it after he had been positively told by several accomplished microscopists that they had seen it resolved; equivalent to saying that they did not know what they had seen so well as Dr. H. did himself; and since that time Dr. Hagen has said that *he* had seen the *true* lines of the nineteenth band with a Tolles' $\frac{1}{10}$th. It of course is a question what constitutes a resolution, but he did not suggest that. I have defined my problem, and submit it to the microscopical world for their approval or rejection.

It is a misfortune that so much time elapses between the making observations, and writing and the publication. My inquiry of Dr. W. was written in September, 1870, though it was from causes beyond my control in my hands until November, and it was not published until March. My question referred of course to the time (May, 1869) when Dr. W. tried the Tolles' objectives. He replies (April?) 1871, stating what the objectives will do that he has then. If he had delayed until July, a change had occurred. He has now ascertained that the Powell and Lealand so-called $\frac{1}{8}$th objectives are really $\frac{1}{10}$th; the so-called $\frac{1}{8}$th is a $\frac{1}{13}$th;* that his description of the appearance of the true lines of the nineteenth band on *his* plate is not a correct description of the appearance of lines of the same band on the plate *I used*. All these facts, I presume, Dr. W. will in due time communicate to the public.

In conclusion, Dr. Woodward is entitled to the thanks of all microscopists, who may undertake the excessively difficult task of resolving these lines, for the admirable manner in which he has described

* Objectives are named when adjusted for uncovered objects, a fact not generally known by purchasers. The power increases, *i. e.* the focus is shorter as the collar is turned to work through the covering glass.

the difficulty of the undertaking, and the obstacles they must encounter. His paper should be read in connection with Dr. Pigott's paper in the 'Monthly Microscopical Journal' for June, 1870. The two papers afford the most complete account of these wonderful lines that I have yet seen.

CHARLES STODDER.

PROCEEDINGS OF SOCIETIES.*

ROYAL MICROSCOPICAL SOCIETY.

The first Evening Meeting will take place on Wednesday, the 4th inst., when the President will read a Paper.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

July 13th.—Ordinary Meeting. Mr. J. J. Sewell, Vice-President, in the chair.

Messrs. Ireland, D. B. Friend, and Dr. Tuthill Massy were elected ordinary members; and the names of seven gentlemen were proposed for election in August.

Mr. Gwatkin reported the receipt, for the Society's album, of seven very beautiful photographs of microscopic objects, made and presented by Dr. Hallifax, including sections of proboscis of blow-fly showing the rasping teeth, poison bag of spider, teeth of medicinal leech, &c., and a water-colour drawing, by Mr. Penley, of Swanbourne Lake, Arundel, from a sketch taken by him on the occasion of the annual excursion.

Votes of thanks were passed to these gentlemen.

Mr. H. C. Malden gave an account of the great difficulties he had encountered in killing a female puss moth, until she had laid her eggs. Apparently killed on a Friday, after laying 175 eggs, she recovered, and though repeatedly, to all appearance, killed on that and the three following days, she did not die until she had laid in all 298 eggs. Many examples were given by the gentlemen present of the extrusion of eggs by moths, not only before death, but even *in articulo mortis* or when the thorax was stiff, and to all intents dead; so great is the effort of nature to propagate the species.

Mr. Wonfor then read a paper "On the Annual Excursion to Arundel, on June 30th," in which the chief incidents of the day, and the various objects seen and obtained, were very graphically and happily described, and especial reference made to the courtesy and hospitality of the Mayor of Arundel (W. W. Mitchell, Esq.), who invited them to luncheon, and of his Grace the Duke of Norfolk, who granted permission to see the gardens and private grounds attached to the castle.

Votes of thanks were passed to the Mayor of Arundel and to his Grace the Duke of Norfolk.

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'

July 27th.—Microscopical Meeting. Mr. J. J. Sewell, Vice-President, in the chair. Subject, "Pond Life."

Mr. R. Glaisyer reported the receipt, for the Society's cabinet, of twelve slides from Mr. C. Neate, four from Mr. Gwatkin, and four from Mr. Wonfor.

Votes of thanks were passed to the donors.

Mr. Robertson announced that a dip made in the moat at Plumpton Place revealed the presence on the American weed *Anacharis alsinastrum* of *Cristatella mucedo*, besides several mollusks, larvæ of ephemera and caddis, four species of Planaria, two species of water beetles, Daphnia, &c.

Mr. Wonfor remarked that though there was not time to go to the marshes when at Arundel, he had made a dip in the lake at Swanbourne, and obtained various desmids, including *Euastrum*, *Micrasteria*, and *Closterium*, several of the commoner diatoms, Rotatoriæ, Flosculariæ, &c., and globules of *Chara vulgaris*, containing spermatozoids, some of which he had mounted for the cabinet. From a pond near the Hassock's Gate Station, in addition to various forms of *Daphnia*, *Cyclops*, &c., he had obtained young tritons, which exhibited the circulation of the blood very beautifully; plenty of *Hydra viridis*, some of which showed developed young hydra attached to the parent, and *Planaria*. On Monday, Mr. Sewell and he, upon the occasion of going to Lewes to assist at a conversazione of the Lewes Natural History Society, had obtained in the marshes, at Southover, plenty of *Volvox globator*; on the frog-bit egg masses of different mollusks, some of which were so advanced that the young mollusks might be seen through the jelly-like substance enclosing them; a few specimens of *Hydra fusca* and *vulgaris*, red and other water spiders, &c.; and that afternoon, from a pond on Furze Hill, he had obtained plenty of *Volvox globator* in all stages, several varieties of *Daphnia*, *Pleuroxus*, *Alona*, *Rotatoria*, &c., as well as a *Melicerta*, *Spirogyra*, and many other minute organisms which he had not time to identify. He had never seen a pond so rich in Volvox as the one he had visited that afternoon.

After a discussion on the nature and generation of Hydra and Volvox, in which Messrs. Sewell, Wonfor, Robertson, and Dr. Hallifax took part, the meeting became a conversazione, when

Mr. R. Glaisyer exhibited various *entomostraca*, including *Daphnia pulex* and *D. vetula*, Planaria, and *Anguillula*.

Mr. Sewell exhibited *Volvox globator*, *Cyclops quadricornis*, &c.

Mr. Wonfor exhibited *Hydra viridis* in different conditions of development, *Volvox globator* in different stages, *Spirogyra*, *Hydrachna*, &c.

It was announced that the subject for the next Microscopical Meeting, August 24th, would be "Polyzoa"; and that at the next Ordinary Meeting, August 10th, Mr. Wonfor would read a paper on "Is *Bombyx callunæ* a variety or a species?".

South London Microscopical and Natural History Club.*

An Ordinary Meeting of this Club was held on Tuesday evening, July 18th, at Glo'ster Hall, Glo'ster Place, Brixton Road. Henry Deane, Esq., F.L.S., in the chair.

Mr. Britten, F.L.S., of the Herbarium, Kew, read a paper "On the Work of Local Societies, especially in connection with Botanical Science." The following is an abstract of this paper :—

The primary duties of local societies are briefly these :—to bring together those persons, residing in a given district, who have paid attention to any group of natural objects, that they may, with mutual advantage, compare specimens and notes; to encourage others in the formation of similar tastes, and to put them in the way of following them out in the manner most likely to be of practical use to themselves; and to investigate, as far as possible, the natural productions of the district, either with a view to the publication of a local fauna and flora, or merely for the purpose of self-instruction. The mere fact of joining such a body indicates a taste for the study of nature, which is in itself a good sign, but it must be remembered that no mere accumulation of money can make a local society successful: you may be rich in cash, but bankrupt in matters of greater value; while if your storehouses of knowledge be full, empty coffers will matter little.

A book upon the flora of Surrey was published eight years ago by the Holmsdale Natural History Club. It may be considered a fairly complete work, but one point in which it is deficient is its want of historical interest. It is exceedingly interesting for a botanist to note the influence of civilization upon the flora of a region. To the antiquarian, what memories of old times are conjured up by the records of the fathers of English botany, of the London plants such as they were in those days; when we might find on our evening rambles in the fields near "a theatre by London" (that is, the first public theatre in London, built in Shoreditch about 1570) wonderful double buttercups; when Penny-Cress grew in the "streete of Peckham"; when the small autumn hyacinth grew "upon a banke by the Thames side between Chelsey and London." One can picture to oneself Gerarde poring over the plants growing in his large garden in Holborn "within the suburbs of London," or John Parkinson, the king's herbarist, whose garden was in Long Acre, going down to Westminster to inspect the famous collection of carnations of his friend Ralph Tuggy, and one is tempted to wander off into speculations as to what these worthy men of bygone days would say were they suddenly to return to life and see all the changes that have taken place since that time, or what we should do or say if put back in an equally sudden manner for 300 years or so! This question of "antiquarian botany," as it is sarcastically called, shows us the immense influence exercised by man upon the flora of a country, simply in a destructive manner. There is no need to go back to Gerarde and Parkinson for this: fifty years ago interesting plants grew at Battersea, and some of

* Report by T. Hovenden, Esq.

them even yet hold their ground. The White Meadow Saxifrage, for instance, appears on the grassy banks surrounding the ditch which runs through part of Battersea Park, just as it did in days before the park was thought of. A list of Forest Hill plants, only forty years ago, contains many even rare species, but they have all doubtless long since perished. But it must not be supposed that it is only by its destructive agency that the advance of civilization affects the flora of a county or country. Surrey is especially notorious as a county in which, through drainage or other circumstances, fresh plants have been introduced. A long list of plants which appeared on some waste grounds at Wandsworth, where the sweepings of Watney's Distillery had been deposited, was published some years since; and a similar list of foreigners at Mitcham has appeared more recently. Most of these plants merely appear for a year and fail to perpetuate themselves; others last for three or four years, and then disappear, while a few find their new locality suited to their permanent existence, and eventually claim admittance to our flora. Here we have a branch of observation well fitted to occupy the attention of a local society, and especially of a society in the neighbourhood of London. So, if we have to lament the disappearance of our plants, we have received others in exchange, equally worthy of notice; and although we cannot botanize in the "pasture and meadow grounds about Pancridge Church," or expect much to reward our search in "the village neere London called Kentish Towne," or the "bankes about Pickadilla," we have full compensation in the facilities afforded us by our numerous railways, which enable us to visit places where bricks and mortar have not as yet extinguished plants, with as little trouble as our old writers found in arriving at their chosen localities for "herborizations." That we have full compensation as to number of species may be gathered from the 'Flora of Middlesex,' where it is stated that, although no less than 58 species are probably extinct in that county, 91 are catalogued as more or less perfectly naturalized, to say nothing of 120 which have been observed in a subspontaneous condition, although as yet they have not succeeded in establishing themselves.

Another important object of the club would be systematic work. This is absolutely necessary for the results to be of any practical value, and I should advise that the observations of the members should be recorded, for comparison with the notes of other observers. All malformations, and deviations from typical forms, should also be carefully noted; and the times of the flowering and pliation of plants should be entered in a calendar, and may be useful for comparison with past records. Lists of mosses and fungi (which do not appear in the published 'Flora of Surrey') might be compiled by the members. Should the club publish transactions or reports of proceedings, it is most important that they should be confined strictly to local matters. In connection with local lists of plants, it would be most desirable to ascertain the local names, as well as any superstitions or traditions locally connected with them, more especially as these latter are fast dying out. I sincerely hope that the club will flourish, and that it

will not be an excuse for spending an evening in pleasant and intellectual chat, but will lead to actual work being done by the members, individually and collectively. It is just in proportion to the amount of work done that the Society ought to and will flourish, and while feeling sincerely sorry that I have been able to say so little which has been worth your attention, I shall also feel that my time and yours have not been quite wasted if anyone should be urged by my remarks to assist with greater energy in promoting the work and welfare of the Society.

Mr. Deane remarked that the paper they had just heard was, in his opinion, a most valuable one. With regard to the extinction of plants, and the introduction of fresh ones, he might mention that when he first came to Clapham, there were at least six varieties of ferns to be found on the Common. There was now, he believed, only one kind, all the rest having become extinct. He believed also that the weed *Anacharis*, which now grew to profusion in the ponds on Clapham Common, had been introduced within the last few years. In a book which he had lately seen, it was remarked that many years ago a gentleman at Wimbledon grew a number of beautiful tulips, of which he was very proud. He believed that the wild tulip was now to be found on parts of Wimbledon Common; this would probably owe its introduction to the number of tulips kept by this gentleman, some of which had doubtless gone wild and spread over the Common. Mr. Deane concluded by moving a vote of thanks to Mr. Britten for his valuable and interesting paper, which was unanimously accorded.

Six members were balloted for, and duly elected, and the certificates of four new members were read by the Secretary.

Excursions were announced, on July 29, to Barnet (for Totteridge), and on August 12, to Thames Ditton.

The meeting then resolved itself into a conversazione; a paper having been announced for the next meeting (on August 15, at half-past 7 o'clock in the evening), by Dr. Hector Helsham, "On the Employment of the Microscope in Analysis."

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Fig. 1

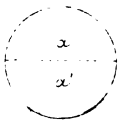
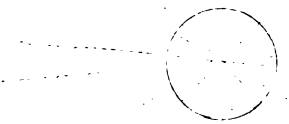


Fig. 2



A.

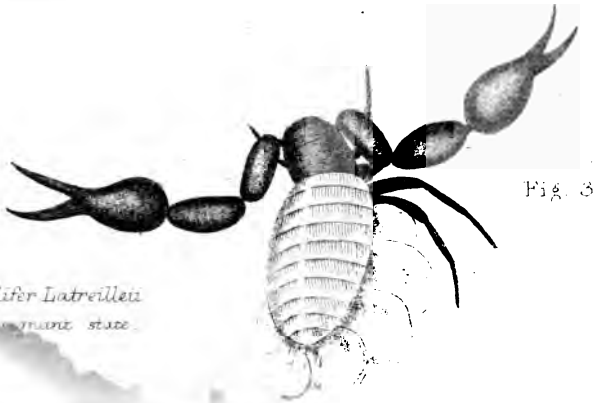


Fig. 3.

Thelifer Latreillei
Segmant state

B

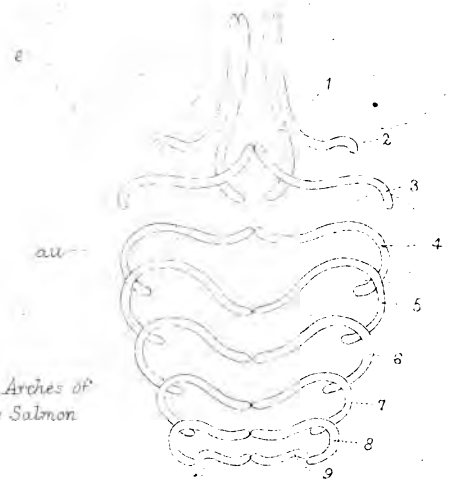


Fig. 4.

Faecal Arches of
Embryo Salmon

THE

MONTHLY MICROSCOPICAL JOURNAL.

NOVEMBER 1, 1871.

I.—*An Incident in the Life of a Chelifer.*

By S. J. McIntire, F.R.M.S.

(*Read before the* Royal Microscopical Society, *Oct.* 4, 1871.)

Plate CII.—B.

For some time past, as some readers may be aware, I have been watching the habits, whenever I got opportunity, of the British Pseudoscorpions, and the results are recorded in the Journal of the Quekett Microscopical Club and the pages of 'Science Gossip'; but lately a circumstance happened in relation to the subject which may be worthy of more special mention.

About April last I procured from Theale two chelifers, one of them full-sized, and the other a young one. I secured them, as is my wont, in one of the cork cells already brought before the notice of microscopists, and which I find indispensable to prolonged observation of such creatures. The large one perambulated the cell with considerable activity, but the small one, with commendable prudence, selected a crevice in it, rather out of the track taken by the adult specimen, and here it abode quietly. Now and then its peace was disturbed by the too near approach of the large chelifer's claws, but it cunningly evaded their grasp, and settled down again when the danger was past. As I dieted them upon their proper food, Poduræ (of the genus Lepidocyrtus, the common sort of which I was able to obtain a supply of at the time), they throve well, and about the middle of May the growth was noticed upon the underside of the larger one's abdomen of the egg-case, leading me to expect a repetition of former experiences, namely, that I should soon have an addition to my stock, if all went well, of seventeen or eighteen young chelifers, which, on extricating themselves from the egg-case, would climb their mother's back, and there seat themselves, secure from most enemies, after the manner of true scorpions, as Natural History books tell us.

But accidents will happen; and so when the egg-burden was of full size, and the shape of the young chelifers therein could be roughly traced out, the mother cast her load off. Whether the young chelifer, whose claws were daily gaining strength, had attacked her in her weak condition, or whether I had disturbed her

in roughly agitating the cell,—for either of these causes is sufficient to produce such a result,—I cannot say. I found her load appropriated by five or six mites (apparently allied to Gamasus), under whose active attacks it soon disappeared entirely, while she, in a very emaciated and excited state, was hurrying to and fro in the cell. To calm and soothe her agitated feelings, I introduced some five or six lepidocyrti, which quickly fell victims to her appetite, and afterwards she appeared decidedly better.

The months of June and July passed, and she lived on, and quite recovered meanwhile. At the beginning of August, however, to my great surprise, a second egg-burden began to show itself on the same chelifer. It went on rapidly increasing in size till about the 8th August, when it ceased to grow larger, though the progressive development of the contents inside it might be observed daily.

Lest any mischance like to the other should happen, I carefully removed the suspected young chelifer to a solitary cell, and was very careful in handling that containing the pregnant one. A rough sketch of her appearance at this period is given* (Plate CII., Fig. B).

But all in vain, for during the night of the 17th August, just when I was expecting, from the advanced development of the contained young, that the experiment would be successful, the irritable mother again detached her burden from her body. While I write, some twelve hours afterwards, the contents of the egg-case seem still alive, for slight movements within can be detected; but yet I doubt if the attacks of predaceous mites alone will not prove too much for the further development of the young, even if the separation of the egg from the body of the chelifer is not fatal to its development.†

The fact of two broods of young, for such I consider it to be, from a chelifer in one summer is, I think, curious and worthy of attention; moreover, the fecundation of the female is an obscure subject. I must conclude it took place previous to April, since I quite exculpate the little chelifer, which was her companion for a long period, for the obvious reason of its youth.

Beyond noticing these points, I will not hazard any speculations on them.

Sir John Lubbock says, if I may be permitted to quote from his letter, "The case seems like one of Parthenogenesis, or perhaps the spermatozoa are retained awhile, as in the bees, &c." The same authority has written a paper in 'Phil. Trans.,' "On the Development of the Egg in the Annulosa," containing a vast amount of matter bearing upon the subject, which those who feel interested should read.

* I believe the species is *C. Latreillii*. † This proved to be the case afterwards.

II.—*On the Form and Use of the Facial Arches.*

By W. K. Parker, F.R.S., President R.M.S.

(*Read before the* Royal Microscopical Society, *Oct.* 4, 1871.)

Plate CII.—C.

Having had an unusual amount of leisure this summer, I have been able to work with the microscope once more, and thus be the recipient of no little pleasure and profit; but, as my time is very swift-winged, it is not proper that I should run from one pretty thing to another. I have had, this time, one subject—the Salmon's Skull.

Those who consider the salmon from merely a dietetic point of view will be shocked to hear that my friends, Messrs. Waterhouse Hawkins, F. Buckland, and Henry Lee, have, together, supplied me with some two hundred specimens. These, however, were not full-grown individuals, but fry and embryos, as yet unhatched. These last have lost their chance of living as salmon, but I hope that they, many of them, will live for ever in the Transactions of the Royal Society; their portraits and descriptions of their personal appearance will be offered to that great good mother of all our Scientific Societies.

It occurred to me, however, that a sketch of the face of one of these water-babies might be acceptable to this pleasant daughter-society.

And here let me say that, when once we know all about the face of infantile salmon, we shall be well prepared to discuss the form of the first foundation of our own face and features. I have not made many alterations in my mode of working this time, but one or two "wrinkles" have been developed.

Firstly, it is better to preserve the eggs and fry in strong spirit, and then to place them in a solution of chromic acid for a week or two before they are dissected; except in some instances, when I want to use high power on thin slices as transparencies, I eschew glycerine. It is better to keep the little preparations in a watch-glass, still preserved in a solution of the acid. This saves them from losing their good yellow colour; in glycerine they become bluish-white, and are bad for examining as opaque objects.

Another thing is the comfort of using only clean water; great irritation of the nerves, to say nothing of the temper, is apt to be produced by the discomfort of feeling one's fingers sticky when working with the glycerine. This is no little matter to a worker with the microscope, for the eyes and brain become intensely weary in such sharp-sighted researches, and the least interruption is apt to injure the calmness of the observer when he is highly strung. One most excellent effect of the chromic acid is, that it preserves, and even increases the lilac tint of hyaline cartilage; that it makes

the soft brain substance as solid as cheese without shrinking it; and that it gives a rich umbre tint to thin laminæ of bone, so that in opaque sections the finest layer—the merest trace of a membrane-bone—can be seen. I have tried a solution of chromate of potassæ with a little sulphate of soda added, as recommended in 'Stricker's Histology,' but I have not found any particular advantage in its use above that possessed by the acid.

Now for the facial arches. The young salmon has one more arch in front of the mouth, and one more behind, than the frog, that is to say, the larval frog: it has, under the head, nine arches in all, two in front of the great mouth-slit, and seven behind it. The first arch, or pair of rods, is the trabecular arch formed by the "rafters of the cranium"; the second is the pterygopalatine; the third, the mandibular; the fourth, the hyoid; and the remaining five are the branchial. The last arch is imperfect and functionless as to respiration.

Speaking of the science of "form," or morphology, let me say, in passing, that it would be a very simple matter if the primary form were fixed; but this is seldom the case, and the original parts undergo a large series, in many cases, of changes, both in form and tissue. This is the case most remarkably in the facial arches of the osseous fish, especially in the two in front and the two behind the mouth. Yet the primary form of these nine arches is the same, as my simple diagram will show (Plate CII., Fig. C).

My earliest observations on these have been made upon very young, thin, unsymmetrical embryos, with a rudimentary solid heart, and with the head flat at the top, and just projecting free from the yolk-membrane. The arches were distinguishable by being granular, but hollow, lying in the midst of, and enclosing, nearly liquid protoplasm. The foremost point most forwards, below, and the hindermost are placed almost transversely across the rudimentary throat; but they all have one shape, *viz.* that of the letter S, the upper part being most hooked inwards.

The first pair, the "rafters," together, have a lyre-shaped appearance as they diverge a little in front, and are strongly bowed behind. The next, or palatine pair, are at first merely semicircular, but they become S-shaped afterwards. All those behind the mouth have a remarkable similarity of form, although the two first of these are larger than those that follow; they all gradually decrease in size, from before, backwards. In the vertebrate animals, generally, these arches have the same form, that is, as far as our researches go; the amount of modification possible is therefore something marvellous. I was not at all surprised to find this S-shaped hooked form of arch in the gill-apparatus of the fish, because there it is persistent, and the inturned tops of the bars are bare of *gills* and carry teeth, which are antagonized by the teeth of

the fifth or gill-less arrested arch. Here the primordial form serves throughout life, and is very gently specialized for life-function. But it was the first pair which most struck me with the beautiful *prospective harmony* between morphology and final purpose, for the same curve inwards at the top, which is so apt for the formation of the crushing apparatus of the fish's throat, here serves to wall-in the pilnitary body, and thus form the primordial "Sella turcica" or *Turkish Saddle.* Again, the next arch, which crescent-like, forms an elegant ledge for the huge eye-ball to rest upon—this arch must needs, as soon as it is freed from the pressure of the precocious visual organ, curve itself inwards at the top. By doing this, it exactly applies itself to the front edge of the succeeding arch, to which it is soldered in a week or two after hatching. The arch of the lower jaw and the arch of the tongue have the same advantage in the upper hook, and all the secondary attachments and delicately beautiful adaptations, as they become specialized, all these, I say, give voice to the morphological importance of the primary curve. It would be endless to go into the *use* of the facial arches in the various tribes, for, when there are no gills developed, as in reptiles, birds, and mammals, the two pairs of horns attached to the bone of the tongue (hyoid), the arch of the lower jaw, the arch of the palate, and all the base of the nasal septum, and of the skull itself, as far backwards as to the exit of the optic nerves —all these parts are derived from the simple S-shaped facial rods. But there is an exquisite instance of special use which I cannot pass over; it is in the class of birds. In these, as in all vertebrates above the amphibia (newts and frogs), the only gill-arch developed is the first, and this is gill-less, but is made to subserve other functions. In most birds, this arch reaches as far as the occipital plane, but in humming-birds and woodpeckers these horns are of extreme length and slenderness, and reach as far as to the fore-end of the cranial roof. These elongated rods form the skeleton of the long worm-like protrusible tongue, and enable it to be shot out without a moment's notice, so that the nimblest of insects are caught "or ever they are aware." A function so new in a gill-arch would seem to ask for a large amount of metamorphic change of form. It is not so; this arch in those birds retains exactly its primordial curve. We must still study *form* free from all final purpose, bias, and preconception; but a new and delightful phase of teleology will set in when the laws of *form* have been mastered. A man may run whilst he is reading the large plain characters in which final purpose is written, but he must be as good a *sitter* as the best hen in a farmyard if he would add anything of value to the science of *form.*

III.—*On the Angular Aperture of Immersion Objectives.*

By Robert B. Tolles, of Boston, U.S.

Plate CII.—A.

Fig. 1 represents a section of two hemispherical lenses balsam-cemented, with a diatom or other small object at the centre, together constituting a nearly homogeneous transparent globe.

Fig. 2. The same, represented as (in section) the front lens of an immersion objective, *i. e.* as to the part *a*, while the portion *a'* corresponds to the last (front) lens of an immersion condenser, both much exaggerated.

In Fig. 2, rays are traced as immergent and emergent at a perpendicular incidence, and therefore without any bending at either surface.

The case is thus completely simplified, and the fact is evident enough that the rays traversing the balsam-mounted object and emerging at the upper surface of the front lens *a*, have materially more than 82° maximum angle. In Fig. 2, the courses of extreme rays for 90°, 120°, and 170° respectively are traced.

In each case the real angle of the *immersion* objective would be the same as the angle of the appropriately applied systems above taken separate from the front lens measured as and constituting a *dry* objective, and (for the sake of simplicity of the case) adjusted for a *dry* object.

An objective, such as is above indicated, inclusive of the front hemisphere *a*, of course could not, as adjusted, work dry, or only so far as actual contact of the object with the plane surface should happen.

On the contrary, the objective used in the experiments described in my communication to this Journal, July, 1871, did work as a dry objective, and of 170° incident pencil, but by construction was limited to about 220° transmitted pencil when the first plane surface was eliminated, or nearly so, by water in the interspace.

The above diagrams and comments are given, not from actual trial-proof, but as an illustration too clear, perhaps, to need the demonstration of experiment.

Let this be added, however,—"No one will have the hardihood to" deny that an object homogeneously cemented centrally between the hemispherical lenses *a a'* can be seen (looking through the sphere diametrically with a simple magnifier) *from every point of view*, thus giving "image-forming rays."

The case is totally and most obviously applicable to that of the ordinary balsam-mounted microscope object for an aperture far above 82° of angular pencil actually traversing the object and made available in the view to the eye of the observer. For obtainment

of extremest angle, however, let one precaution be taken, *viz.* that *balsam* be used above the slide and balsam below! The case will then correspond to that given in the diagram.

Of course in practice the upper convex surface of the front lens, or system, has a curvature and distance to positively and considerably refract the transmitted rays, but the case I have given is the easiest elucidated.

BOSTON, *August 22nd*, 1871.

IV.—*Note on Pedalion mira.* By C. T. HUDSON, LL.D.

IN my paper on *Pedalion mira*, in the September number of the 'Microscopical Journal,' I purposely omitted to give any sketch or detailed account of the internal structure of this new and singular rotifer, for I had not had time enough to investigate it thoroughly; but as some doubt has been expressed as to whether *Pedalion* is a rotifer at all, I wish to state that it has a trilobed mastax, with a manducatory apparatus similar to that of *Triarthra*, and the usual convoluted tubes carrying at least two vibratile tags on each side, though most probably there are more. I have not seen any contracticle vesicle; but then I have equally failed to see it in *Triarthra*, in which rotifer, as well as in *Pedalion*, either the dense corrugated walls of the posterior extremity of the stomach overlie the vesicle, or the vesicle itself (as in *Pterodina*) is evanescent.

Pedalion's muscles are, for its size, enormous; at least two broad and coarsely striated muscles run transversely round the body below the neck, and the longitudinal muscles for retracting the trochal disk are unusually powerful.

I had intended in the course of September to complete my investigation of *Pedalion's* structure; but the creatures diminished in number rapidly throughout August, and have now, I believe, entirely disappeared—to return, I hope, next summer.

V.—*Another Hint on Selecting and Mounting Diatoms.*

Communicated by Capt. FRED. H. LANG, President of the Reading Microscopical Society.

MY paper "On Selecting and Mounting Diatoms," read to the members of the Reading Microscopical Society in October, 1870, and published in the December number of the 'Monthly Microscopical Journal,' has been, I have every reason to believe, of con-

siderable use to amateur microscopists. In last April's number of the same Journal appeared a friendly notice of it in a letter from Capt. Knight, in which, acknowledging the advantage of making classified collections of diatoms, he speaks of the difficulty of procuring the material for so doing, and observes that professional mounters and opticians will only sell you their mixed gatherings, set, as a general rule, in balsam; though occasionally, but seldom, a dry mount may be obtained, when it is an easy matter to remove the cover and select the required forms. He does not, however, appear to think it possible to utilize for the purpose the balsam-mounted material, and I suspect others as well as myself have till now been of the same opinion. My friend Mr. Tatem has, however, turned his attention to the subject, and has discovered a very easy plan for picking out any desired forms from such slides for the purpose of remounting them. He has kindly communicated his method to me, and permitted me to publish it as an addendum to my former paper. Having both of us given it a fair trial we can confidently recommend the plan, which is as follows:—

Place the balsam-mounted slide on the hot plate, and when it is sufficiently warmed tip over the cover by means of a needle; the diatoms will be either on it or the slide, it matters not which. Apply over them at once, whilst still on the hot plate, a drop of turpentine, remove the slide to the stage of the dissecting microscope, and add more turpentine. Have ready a clean slip of glass on which has been placed a drop of turpentine. In the case of large discoid and other forms, having applied plenty of turpentine, they can be easily transferred by means of a fine sable-hair brush from the original slide to the pool of turpentine on the clean one. In the case of finer forms it is better to place less turpentine on the original slide, collect the diatoms into a heap, allow the turpentine to dry a little, and then by a twist of the brush to transfer them *en masse* to the new slide. In either case, having got them there push them together and mop up the superfluous turpentine, and then, still under the dissecting microscope, slant the slide by placing a piece of folded paper under one end, and apply a little benzole either by means of a clean brush or glass rod immediately above them, that is, on the end of the slide that is raised, and allow it to float *gradually* over them, care being taken that it does not flow with too great a rush and carry away the diatoms with it. Repeat this process some half-dozen times, till the whole of the turpentine and balsam has been washed away, and till the valves are left dry and black after the benzole is evaporated. They can then be transferred in the usual way to any other slide, and even with greater ease than from an ordinary dry gathering. I may as well add that if gum has been used to fix the diatoms, it may be found that some of the valves, especially the discoid ones, remain obstinately adherent

to the glass after the turpentine has been placed over them. In such a case, the process, as above detailed, must be carried out on the original slide, and then, after the benzole is thoroughly evaporated, water must be applied two or three times in the same way as the benzole for the purpose of washing away the gum and freeing the diatoms, which can then, when dried, be lifted one by one and transferred in the usual manner.

By this simple and easy method we can not only select from balsam-mounted gatherings any particular valves we may require, but we can reset any spoilt or unsatisfactory mounts of our own.

VI.—*The Monad's Place in Nature.*

By Metcalfe Johnson, M.R.C.S.E., Lancaster.

Plate CIII.

Among the objects in the department of Protozoa one sees at the same time groups of organisms having attained a certain degree of perfection, and other living forms in a more elementary condition. The object of the present remarks is to show a connection between the earlier forms, which we call Monads, and those higher and more complicated organisms at present recognized under the name of Infusoria, Mucedinæ, Confervæ, Oscillatoria, &c.

The following record of experience, taken together with the previous papers in 'Monthly Microscopical Journal,' will be found to contain some evidence bearing on the tendency of certain or-

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DESCRIPTION OF PLATE CIII.

Fig. I.—A tubule of Vaucheria, containing :—*a*, mass of chlorophyll; *b*, bursting of tubule and discharge of Monads, &c.; *c*, a green oval Gonidium; *d*, two Monads moving within the tubule; *e*, an immature Pseudogonidium; *f*, an Euglena; *g*, a mass of chlorophyll giving off, *h*, a globular extension of the primordial utricle; *i*, the terminal vacuole of the tubule containing Monads in active motion; *k k*, two Pseudogonidia being discharged from the tubule; *l*, a transparent primordial utricle, or empty cell-wall; *m*, the same, containing chlorophyll; *n*, one of these in the act of bursting, and liberating, *p*, the Monads, which at once take an independent cyclical movement.

„ II.—A piece of *Conferva rivularis: a*, the hole through which the Euglenæ, *b*, are escaping. This observation was made April 11th, 1869.

„ III.—A pin-point Monad in its transformation to Confervoideæ.

„ IV.—A pin-point Monad in its transformation to Penicillium.

„ V.—A pin-point Monad in its transformation to Chlorococcus: *a*, Gonidium; *b*, Gleocapsa; *c*, commencement of Soridium; *d*, *e*, bifissation; *f*, division into 4; *g*, division into 16; *h*, formation of Thallus or Apothecium.

„ VI.—A pin-point Monad in its transformation to Euglena: *a*, Gonidium; *b*, oval cell with vacuole; *c*, Euglena; *d*, with filament; *e*, intersuscepted form.

[Fig. VII.

ganisms to develop progressive change from a lower to a higher form of structure, and will in this instance tend to establish the probability that the Monads which we meet with in various states are the sources whence spring some of the more developed forms of life to which I have referred.

In the air caught by trickling water over a sheet of glass,* I find a large number of Monad forms about $\frac{1}{8000}$th of an inch in diameter.

In November, 1867, I found bodies having the same appearance and the same cyclical movement, moving within the tubules of Vaucheria (Pl. CIII., Fig. 1. *d*). In April, 1868, I confirmed the experiences (Fig. 1. *i*). In July, 1870, I saw similar bodies moving freely in the vacuole of a desmid (*Closterium lunula*). In 1867 I watched the primordial utricle in the tubule of Vaucheria protrude from the general mass of chlorophyll, and form a round globular vesicle into which the Monad forms escaped from the mass (Fig. 1. *g*, *h*). I also saw the Pseudo-gonidia attached to the side of the tubules of Vaucheria containing chlorophyll particles and a vacuole (see the diagram, Fig. 1. *k k*). Also I witnessed the primordial utricle of a Pseudo-gonidium burst, and the Monads liberated from the interior, each being transparent, about the size of those found in air, and having their own independent cyclical movement (Fig. 1. *n*, *p*). Now it seems to me that these organisms are in all likelihood similar to those referred to by numerous observers, such as Samuelson, Dancer, Angus Smith, and others of recent date, who have confirmed the experience of Pasteur, which in all likelihood relates to a form of life similar to that to which I have here referred.

Now in the bursting of a tubule of Vaucheria, which I witnessed in November, 1869, the size of the particles varied from visible

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Fig. VII.—A pin-point Monad in its transformation to Infusoria: *a*, granular cell; *b*, showing vacuole formed by contraction of sarcode; *c*, first change to Amœba; *d*, perfect Amœba; *e*, change to Vorticella; *f*, Vorticella; *g*, Kerona. See also Fig. IX., which represents an observation made August 12th, 1871, in which I saw the globular body throw out cilia, and then swim away as a perfectly formed Kerona.

„ VIII.—A pin-point Monad in its transformation to moss: *a*, *Oscillatoria Nigro Viride*; *b*, Lyngbya; *c*, moss cell as given off from the surface in drops of rain.

„ IX.—Change from a spherical cell to a distinct Kerona: *a*, the spherical shape; *b*, throwing out cilia, *c*; *d*, fully formed Kerona.

„ X.—A few Paramœcia (*Kolpoda cucullus*) swimming in a fluid composed of Vibrions and Monads: *a*, Vibrions; *b*, Monads.

„ XI.—An Infusorium (probably *Paramœcium Aurelia*), showing cilia placed diagonally: *a*, ventral; *b*, dorsal view.

„ XII.—A mass of pin-point Monads: 1, becoming, 2, Vibrions; 3, Bacteria; 4, Monads and Uvellæ; 5, Amœbæ; 6, Paramœcia; 7, more highly developed Infusoria, as *Kolpoda cucullus*, &c.; 8, Vorticellæ.

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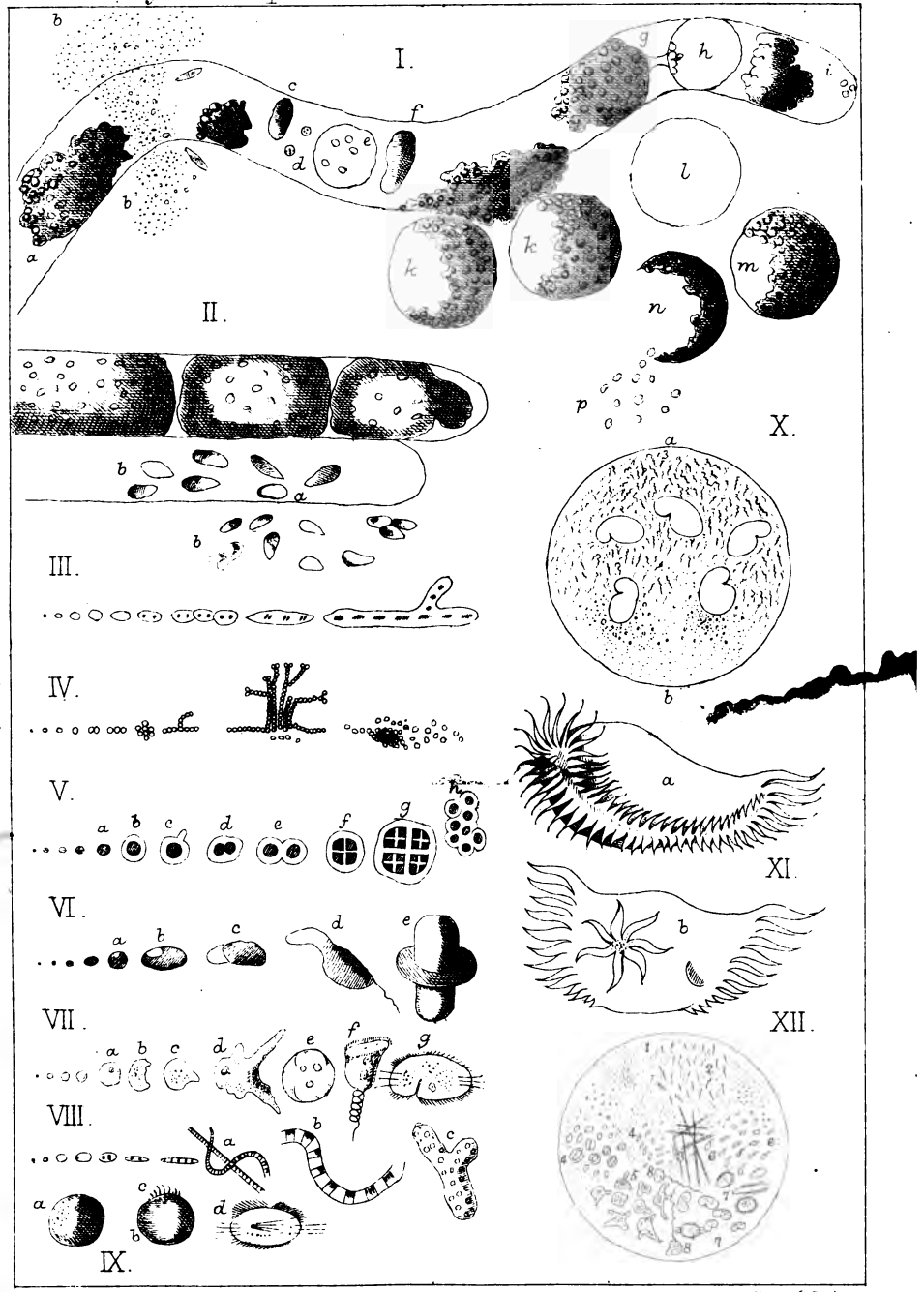
* See 'M. M. J.,' Aug., 1870.



W. West & Co. imp.

Monads.

masses having a green colour, to minute specks which were only visible by scintillating in reflected light (see Fig. I. *b*); and in all examinations of matter containing organisms, certain particles are seen to revolve upon their own axes, and to present what appear to be signs of life. These solutions, if watched from day to day, present objects having the same movements, which gradually develop until they present the same appearance as the Monads before referred to, as being caught from the air and being discharged from the Pseudo-gonidia and the tubules of Vaucheria.

An examination of the experiments recorded in my earlier papers in this Journal shows these same Monad forms as always the first to make their appearance.

In the experiment, March 5, 1868,* bottle *b*, with only a limited quantity of air, develops first Monads and Mucedo on cork; and after more air had been admitted, successive observations showed Monads, Vibrions, and ultimately Paramœcium.

In Experiment *e*, the ultimate development from the same source, but under different circumstances of light and air, was green Gonidia, Euglena, and green filaments.

But in *g* the result was an immense number of full-sized Euglenæ.

The sources of all these developments seem to me (who watched the liquids daily for two months) to be the same minute pin-point Monad which Dr. Bastian refers to in his papers in 'Nature.' An examination of my note-book during the watching of these liquids shows numerous forms which the Monads presented (Fig. XII. 6), evidently transitional from the round pin-head Monad to oval young Paramœcia, until we come to sufficient size to give it a name such as *Kolpoda cucullus*, &c. (Fig. XII. 7).

In the vacuole of Vaucheria I observed that organisms in several stages of development existed in active motion at the same time (Fig. I. *c, d, e, f*), and I have since been able to verify this opinion by observing full-grown Euglenæ in the tubule actively moving; but on April 11th, 1869, I saw the birth of Euglenæ from a tubule of *Conferva rivularis*, and on escaping they took on all their polymorphic changes, and although small in size were full of life (see Fig. II.).

The important deduction from this fact is that there are various stages at which the organism enters upon its independent existence, and it may fairly be presumed that as the stage is early or developed, so will the amount of heredity vary, and the tendency to resist the *choses extérieures*, or accidents of life, become greater.

The "pin-point" Monad being in an earlier stage, and possessing less heredity, may, and most likely does vary more under the influence of these accidents of life, such as light, air, &c.

* 'M. M. J.,' Aug., 1869.

It has been noticed that in almost all substances or liquids examined, the first evidence of life is found in the movement of most minute bodies. An instance of this and the subsequent development is well shown in the following case:—

On the 1st August, 1871, I took a piece of fluff from under a carpet and book-case, which had for some time remained undisturbed.

On examination, it consisted mainly of the coloured broken pieces of wool from the carpet, as well as pieces of flax, together with the ordinary contents of dust, such as soot-flakes, splinters of flint, and amorphous particles—starch grains, &c.

I then took six drachm-bottles, put a small piece of the fluff into each bottle, and half filled it with water. (This was taken from the tap in which no organisms are perceptible, nor do organisms develop from it when placed under similar circumstances to the present experiment. It may therefore be inferred that any living things which should from time to time appear, were due to the dust, and not to the water). The bottles were then tied over with carded cotton, and examined as follows, with the results named.

The bottles were marked A, B, C, D, E, and F.

August 3rd (after two days).—Nothing but Brownian movements visible in bottle A.

August 4th.—Bottle B, unmistakable movements among the particles in a cyclical direction. One particle circulated first in one direction, and afterwards in another. The larger particles seem to be moved by some smaller but invisible objects.

August 5th.—Examined bottle C, and found very active Monads, apparently attached by a pedicle to some particle of amorphous matter.* Movements undoubtedly voluntary. The appearance of the Monad is round at first, but apparently changing shape from time to time, as if of an amœboid form. The size, under 300 ×, is of a good-sized pin's head. Under 700 ×, I found one having the movement and appearance of a young Paramœcium.

August 8th.—Examined bottle D, with similar results.

„ 10th.—Examined bottle E: the Monads smaller.

„ 12th.—Examined bottle F, and found most unmistakable *Kolpoda cucullus;* movements cyclical and rotating on their own axes, apparently about $\frac{3}{8}$ inch long by $\frac{1}{4}$ inch in diameter when magnified 250 times (about $\frac{1}{600}$ inch in length).

One or two points here require observation. It is a question *sub judice* as to the cause of the Brownian movements. I am inclined to the belief that they are due to the presence of life in some objects too small for independent vision by the microscope. I have

* My friend Mr. Chantrell, of Liverpool, informs me that he has watched the growth of Vorticellæ, and finds the stalk developed before the head.

often found that liquids which under low powers appeared quite free from organisms have, on the application of higher powers, resolved themselves into moving masses of organisms, and this, I believe, will most frequently be found to be the case where the power is able to detect the life. In the case of August 4th this is palpably the case.*

I am also inclined to the belief that the lower forms of life are amœbiform, from the numerous observations I have made of them when the focus of the instrument has been clearly adjusted.

The cause of the circular movements may be due to cilia all over the body, as shown in Fig. 18, Plate C., of my last paper in 'M. M. J.'

But the cause of the cyclical movements may be explained by an examination I have occasionally made of some form of Paramœcium, of which I have given a diagram in Fig. XI. *a*, *b*, Plate CIII., possibly *Paramœcium Aurelia*. Here the cilia are placed in a diagonal manner across the body, and give it by successive action that peculiar motion which we observe as cyclical, and which was so in the objects from which the drawing was made.

From observations I have made I am induced to believe that the pin-point Monad, when developed under absence of light and only a limited quantity of air, gives rise to the class of plants known as Mucedinæ.†

The results of the experiment, March 5th, 1868, *g*, shows Euglena as the result of cow-dung in much water in free light and air in a tall glass jar, while the same medium in *d*, gives only Paramœcium as the result, the air and light being both applied in a different proportion.

For these and other reasons I am induced to look upon Monas in its earliest forms to be the starting-point whence, when its heredity is small, several products may result, and among the number are Infusoria, Mucedinæ, Euglenæ, Oscillatoria; and since I think there is evidence to show much transmutation of form, the facts seem to me to show that while from the lower Infusoria many of the Rotatoria may be traced, so also from *Palmella cruenta* may be traced Oscillatoria, Lyngbya, and many mosses (see Figs. III., IV., V., VI., VII., and VIII.). A close and frequent examination of plants of *Palmella cruenta* has convinced me that *Oscillatoria Nigro Viride* is simply a developed form of the same plant. Instances are of very frequent occurrence in which the plants are so intermixed, and the cells under the microscope so evidently transitional, that I have now no longer any doubt upon the subject.

* See also Fig. X., in which the liquid was one moving mass of Vibrions and Monads.

† See p. 197 'M. M. J.,' April, 1870. Note of an experiment in which the results of growth in a dark cupboard is compared with that in an open window exposed to light and air.

A similar examination of Oscillatoria with Lyngbya reveals the same transition. Were it not that the forms are too common for a diagram, I have abundant evidence in my notes in proof of my opinion. The point, however, is one capable of investigation by comparatively low powers of the microscope, and may form a very interesting point for young observers to commence a systematic use of microscopical investigation by means of notes and careful drawing. And here I would trespass upon the pages of 'M. M. J.' by offering this suggestion, that young microscopists, instead of using their instrument as a mere toy to look at strange and beautiful objects without system, might confer great benefit upon the cause of science by taking up this subject and carefully working it out. It is a mistake into which many men fall when they suppose that they are unable to help the cause of science by microscopic research. I am convinced that honest and careful tabulated experience is always valuable, and the more so as it is multiplied by many experimenters.

If we look upon the broad face of any landscape we are at once struck with the growth of green verdure upon all surfaces which are rough enough to receive the air-borne seed of plants, and have remained long enough undisturbed to allow of the development of this verdure.

An observation of it under various conditions shows various forms of development, and as it is in most cases evident to the simplest consideration that the product is air sown, so it also becomes evident from the facts before us that the source of it is some matter of constant existence in the air. Now, an examination of the air shows us occasional green Gonidia which manifestly have sprung from lichens or mosses, from the elastic membrane concerned in the bursting of apothecia of lichens on the one hand, and the bursting of tubules of mosses as shown by Dr. Hicks on the other; and the facts adduced in this paper relative to the bursting of vacuoles of Vaucheria, Pseudo-gonidia, and of the tubules of *Conferva rivularis*, give us evidence of a source whence the seeds of these plants may be ejected into the air. This being the case, it becomes necessary to observe the results of this sowing of seed under proper means of observation. I have in my study at the present time a large quantity of green Lyngbya, which owes its existence to a small number of cellules of Chlorococcus, which was sown by the air upon a lint siphon which I used for the purpose of collecting air-borne seeds about two years ago.

In a previous number of 'M. M. J.' I showed how four distinct species of moss gave off similar Gonidia, which produced Chlorococcus and Lyngbya, and every day's experience confirms these observations.

But the experiment upon the fluff and dust as detailed in this paper points more distinctly to the pin-point Monad as the source

at least of Infusoria, and taken in connection with the other experiences seems to show at least a probability that in this minute form a great process of the most gigantic importance is taking place in nature.

I think it is not making too extravagant a demand upon the imagination or the credulity to suggest that here, in the border-land of the two kingdoms, animal and vegetal, nature is performing a vast operation in converting the dead matter of the universe to living forms, by a process of cell-growth which is so universal as to reveal to us some of the workings of a vast law.

We may fairly consider the living cell, in its initial state, to be a vesicle (too minute for the highest powers) which by dialysis absorbs to its cavity the elementary atoms of which the dead organic matter had been composed, and which it is now ready to part with in consequence of its vitality having ceased. Upon this pabulum the living cell grows and increases until it is able to give off proles, either as the pin-point Monad, the visible Monad, from the Pseudogonidium, or the Euglena, born of *Conferva rivularis*, Vaucheria, or some other plant of this nature.

The proles, starting at the most minute point, becomes either the Mucedo (converting by its growth the dying particles of hydrocarbon compounds to the alcohols and æthers of animal or vegetal decay), or the Infusorium (gradually developing to Rotifer, and thence probably to higher organisms), or the beautiful green Euglena (on the rank hotbeds of farmyards and other nitrogenous depôts), or the delicate Chlorococcus (whence arise *Parmelia Collema*, or Lichens hanging in graceful festoons from the "formed matter" of old tree barks), or through *Palmella cruenta*, Oscillatoria, Lyngbya, to mosses, forming those sponge-like reservoirs of moisture which in their decay give a suitable nidus to the air-borne seeds of the sycamore or other trees, as may be seen on the towers of many of our older churches. There many a good-sized tree grows in the débris of vegetable matter mixed with the silica flakes of dust wind-borne from the dry roads.

It will here be seen that this branch of study bears as it were the seeds of one of the most important investigations which can occupy the thought of the reflective. It seems as it were to open out a page of the world's history which shows that from the smallest beginnings the greatest of results follow. It serves to show that there is no part too small to be of the highest importance, and that the decay of nature is but the other side of the portal of life —equally beautiful, equally honoured by the Creator with the most minute detail of perfection. Man is apt to look upon that which is ceasing to possess life as disagreeable, offensive, unworthy of attention—to be, in fact, cast away as used up and belonging to the despised condition of waste. Let such a thinker then turn his

thoughts hitherward for a short time, and learn that nature knows no waste; each particle of which the universe is composed is everywhere and at all times performing a duty and serving a purpose, and though it may change its shape from the beautiful rosebud or the gaudy butterfly to the destructive mildew or the more homely grub or caterpillar, yet here when viewed through the microscopic lens every part is full of beauty and perfectly fitted for its destination, either as a carrier of life or a destroyer of the dead—Genesis and Exodus—I say again, both equally honoured by the Maker, equally perfect, equally beautiful.

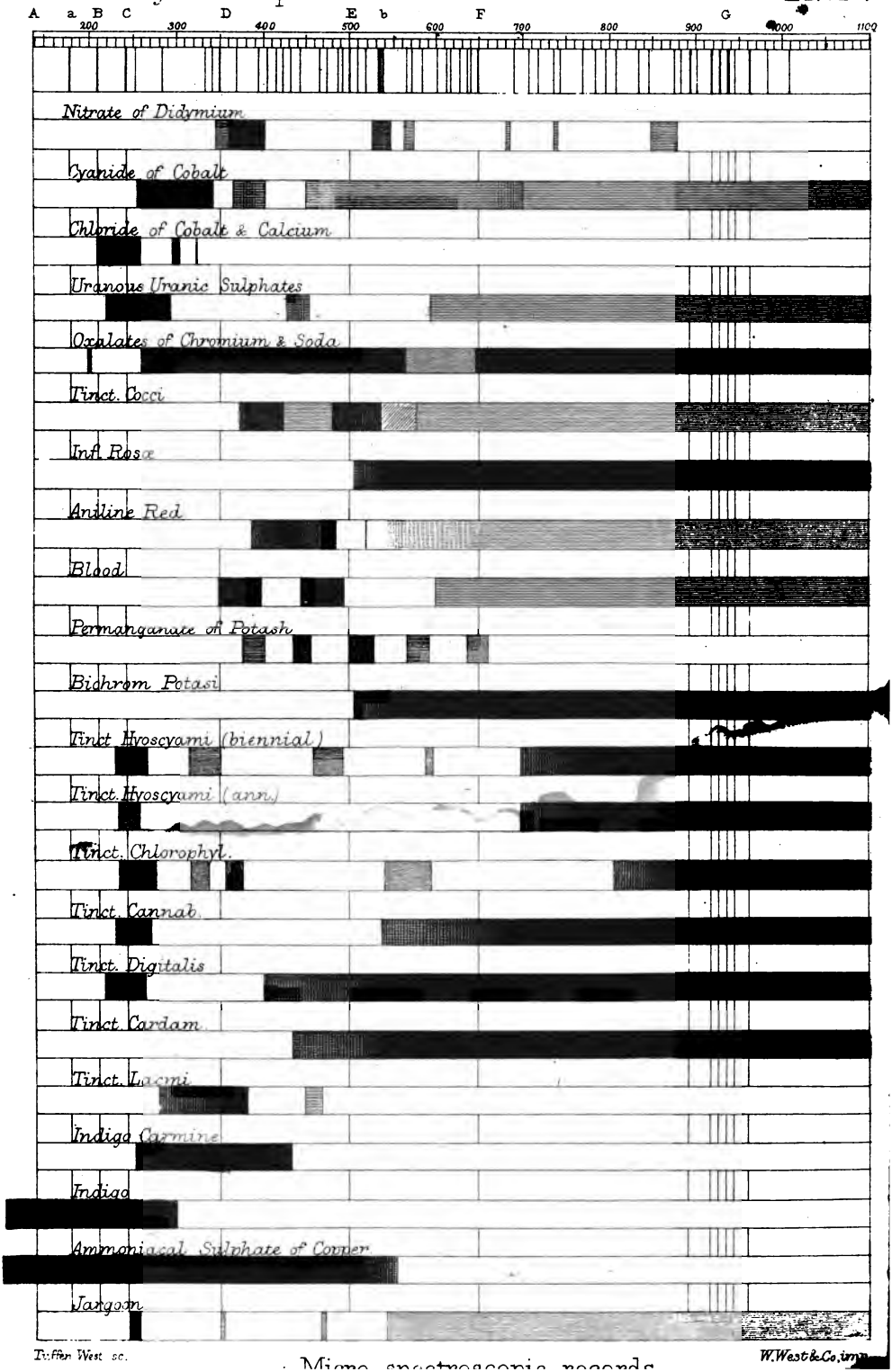
VII.—*Mapping with the Micro-spectroscope, with the Bright-line Micrometer.* By H. G. Bridge.

Plate CIV.

Now that spectrum analysis receives so much attention, as the means of furnishing information in solar chemistry, and also in researches in the spectral lines afforded by the combustion of metals by the electric spark, and the absorption bands given by fluids, &c., the bright-line micrometer so ingeniously contrived by Mr. John Browning has greatly added to the efficiency of the micro-spectroscope.

The devising a reliable means of determining the relative positions of lines and bands given in the spectrum in a direct rigid spectroscope was a problem of no little difficulty, but Mr. Browning has solved it; and while it may be admitted that the use of this micrometer, in order to obtain reliable results, requires some care, as from the nature of the whole instrument it is necessarily delicate, yet mapping by its aid is perfectly practicable, and if attention be given to a few directions which I purpose adding for the guidance of surgeons and others engaged in studying spectral analysis, anyone can furnish himself with maps of the spectra of his instrument, mapping many of the Fraunhöfer lines, and the absorption bands of tinctures, solutions, &c., for reference and comparison. In short, to show how far the use of the micrometer is practicable, it is perfectly feasible to construct a map of lines whose positions shall be correct to the unit of the micrometer circle. The accompanying map is correct for any instrument to this degree—of course this applies only to the lines and such of the absorption bands as are well defined, the rest are as correct as the, in some cases, somewhat nebulous nature of the bands admit of.

It is necessary, when the instructions given in Mr. Browning's pamphlet on the instrument have been attended to, to focus on the object under examination, and that the slit be also focussed by the


A
a
B
C
D
E
b
F
G
200
300
400
500
600
700
800
900
1000
1100
Nitrate of Didymium
Cyanide of Cobalt
Chloride of Cobalt & Calcium
Uranous Uranic Sulphates
Oxalates of Chromium & Soda
Tinct. Cocci
Infl. Rosæ
Aniline Red
Blood
Permanganate of Potash
Bichrom Potasi
Tinct Hyoscyami (biennial)
Tinct. Hyoscyami (ann.)
Tinct. Chlorophyl.
Tinct. Cannab.
Tinct. Digitalis
Tinct. Cardam.
Tinct. Lacmi
Indigo Carmine
Indigo
Ammoniacal Sulphate of Copper
Jargoon

eye lens; and for registering the readings of the lines the direct rays of the sun should be thrown up into the slit, made as narrow as is compatible with sufficient light, and when the prisms are put in place it is best to focus on the E line as sharply as possible, this line being near the middle of the spectrum; this is best done by so focussing as to be able to see as many of the fine lines in the neighbourhood of E as possible. Now by means of the reflector make the bright line or spot of the micrometer visible, bring it over E, just above the spectrum, focus it by its lens so that it is sharp and quite free from flame, and so that a lateral to-and-fro motion of the eye does not displace it by parallax; this is most important, all accuracy will depend upon it. Now see that all the fittings of the instrument are firm and as free from spring as possible, and with a light hand bring the spot over E, taking care that no part of the instrument but the micrometer screw is touched, and do not let the eyebrow touch the eye-piece, as this may shift the spot. Having registered the best reading by many averages of E, that of A B C D and F G should be each *separately* and *directly* determined from E, and bringing the spot back again to E to test its reading. If great care is taken in the first instance in determining the true relative positions of these principal lines it will be well bestowed, and any of the smaller lines can be registered by taking the nearest principal line as a standard.

It may be found necessary to adjust the focus of the micrometer lens when the spot is over the extreme positions; it must then be started again from E as a point of departure.

It will be found on using the instrument another time that the spot will not give the exact registered reading of E; it must be made to do so by a slight side pressure or twist given to the eye-piece cap carrying the micrometer. Hence the importance of having the instrument in its firmest condition before finally determining the reading of E.

The accompanying Plate (CIV.) has been carried out on the plan described above, and explains itself sufficiently.

VIII.—*Some Remarks on a "Note on the Resolution of Amphipleura pellucida by a Tolles' Immersion $\frac{1}{8}$th. By Assistant-Surgeon J. J. Woodward, U. S. Army."* By Edwin Bicknell.

There are some points in the "Note" of Dr. Woodward under the above title, which, I think, will bear a few remarks. I have been much interested for the past three years in comparing and measuring different objectives by different makers, in order to ascertain how near the "nominal power," as rated by the maker,

corresponded with the "actual power" when the magnifying power was measured by any proper method.

In the majority of cases I have found the "error" to be in favour of the objective; that is, the "nominal power" was less than the actual power, and the objective consequently "underrated" in power, sometimes to quite a large amount.

Dr. Woodward in his note is very careful to give the proper rating of the Tolles' objective, after giving its magnifying power at 48 inches distance between the micrometer and screen; he says, "It is therefore of rather higher power than a $\frac{1}{5}$th, but less than a $\frac{1}{6}$th," which is undoubtedly correct.

A little farther on in his note Dr. Woodward gives the power of another objective by means of figures; I allude to the *so-called* immersion $\frac{1}{16}$th of Powell and Lealand. Applying Dr. Woodward's rule to his figures giving the "actual power" of the so-called $\frac{1}{16}$th, would make it "*therefore of rather higher power than a $\frac{1}{20}$th, but less than a $\frac{1}{24}$th;* at any rate Dr. Woodward's figures make Powell and Lealand's objective just four times the power of Tolles'; and according to my way of figuring, the above would be correct, that is, the Powell and Lealand objective would be between a $\frac{1}{20}$th and a $\frac{1}{24}$th, depending upon the position of the "cover adjustment."

I call it nothing more nor less than *deception* in Powell and Lealand (or any other maker) in marking an objective nearly 33 per cent. less than its actual power, thus misleading people who cannot make actual comparisons; and I consider Dr. Woodward guilty of an equal amount of deception in *knowingly* putting forth work done by that objective as having been done by a $\frac{1}{16}$th.

Dr. Woodward says "that the new $\frac{1}{5}$th cannot be claimed to supersede the highest powers at present in use" (meaning the so-called $\frac{1}{16}$th, the $\frac{1}{25}$th, and the $\frac{1}{50}$th of Powell and Lealand, I suppose), "yet nevertheless is not, in my opinion, injurious to the $\frac{1}{5}$th." Such innocence is refreshing; the fact of the $\frac{1}{5}$th not "superseding" objectives of from *four* to *ten* times its power not being "injurious" to it is decidedly rich.

No one, so far as I know, has claimed that Mr. Tolles' $\frac{1}{5}$ths were to "supersede the highest powers now in use"; but if Mr. Tolles, Powell and Lealand, or any other maker should make a $\frac{1}{5}$th that would do better than the best $\frac{1}{10}$ths or $\frac{1}{12}$ths of the present day, it would be "glory enough."

IX.—*Infusorial Circuit of Generations.*

By Theod. C. Hilgard.

The soft and "naked," transparent and really animal* forms here to be considered, have some very striking and peculiar features in common. Their bodies are delicate, transparent, gelatinous, granular, and evidently *sexless*, although studded with reproductive yolks and locomotive molecules of the most varied description. On contact with air, when drying up, they do not leave behind any coherent coat or tissue whatever; but so soon as they are affected by incipient exsiccation, at once, by some sudden internal commotion, as if *melting* away, they become *liquid*, and entirely dissolve into a "sauce" of quite uniform, hyaline molecules, about $\frac{1}{2000}$ line in diameter. They are all evidently *immature* forms, subject to a vast cycle of progressive and retrograde developments, and infinitely multiplying the molecular germs at every individual dissolution.† A little salt, glycerine, or sugar destroys their present form; but they seem to be hardly affected by morphia or atropia, even in strong solutions.

It is this feature of the *non-endurance in drying up*, which renders it at once certain that no such sarcode bodies can continue to exist *in integro*, when exposed to the full heat of summer, on a cracking dry tub, or on a roof, likewise as torrid as a blazing July sun can render it within four weeks. The same applies to all the confervaceous, palmellaceous, protococcous, desmidiaceous, &c., fresh-water spawns, of true Mosses; which, once collapsed by drought, rarely continue growth in a progressive sense. With the exception of their *common* "nostoc" phase ‡ (specially adapted to endure even excessive dryness), they "revive" *only* by starting anew from very reduced, but immensely multitudinous *constituent particles of their*

* *i. e.* exclusive of all the chlorophyll-endowed, silica-coated, and automatous, or cellular cell-like sarcodic bodies, and also the clear and vibrionic forms which belong to the algoid *bryaceous developments*. They are partly classed as green "Infusoria," and also constitute the "Chlorospermeæ" of "Algæ."

† The same doubtless applies to a small "Stentor Roe," seen hovering up and down in water taken from ponds, aquaria, &c. It is of a hazy white colour, scarcely perceptible to the naked eye, and remarkable for never touching the surface. When placed under the microscope, in a drop exposed to air, this animal germ (in shape resembling a Cyprea or a coffee bean) is seen violently throwing open its "cloak" or mantle, exhibiting an intense ciliary (*fingered*) vibratory action, *all over the interior surface*. It then throws out hyaline constitutive brood-balls of various sizes, *each endowed with the same "fingered" action*—(as if "kneaded about" in invisible hands)—and ultimately entirely *flows apart into such fleshy cilia*, on leaving the *intestine* behind.

‡ This form of self-multiplying, serial chlorophyll bead-strings enveloped in a foliaceous slime is common to Lichens (particularly the genus Collema) and various brooding phases of the algoid (*chlorospermous*) moss-spawns. With the Lichens, the internal chlorophyll of their thallus often develops, as is well known, in similar bead-strings, borne on the end of colourless (fungoid) tissue-fibres; in a manner also represented in the anatomy of *Blodgettia confervoides*, in Harvey's 'Nereïs.'

own, which perdure exsiccation. In the class of Fungi we meet with similar examples, as *e. g.* in the case of the yeast-plant, which can endure a considerable degree of exsiccation without impairing the vitality of those cell-*contents*, which actually exercise the fermentive energies and also consume the old cell-coat (*not* common cellulose, by the way) in this process of reviving. Likewise, the vermilion, gummose (*tubercularia*) pimples on rotting Black Oak branches can endure a baking process in the burning sun, but still revive on contact with water. Its spicular "spermatia" (*fusidium*, resembling Naviculæ or magnetic needles) at once assume an oscillatory motion and swell up to the size of those didymous (*trichothecium*) spores which presently stand erect on pedicels, as a pink velvet, in the chinks of the bark and collapse at the first touch of the sun; while their ultimate subcortical development into a mature, "black enamel" Sphæria again perdures in the heat.

Under the circumstances above mentioned, the rain water and dry dust carried by the wind to the roof, and thence collected into a perfectly dry tub, itself standing on a similar roof, within a few days was found swarming with all the minor phases of the Vorticello-planarian germ-developments. Both the bodies and the yolks or gemmæ of the latter occasionally become reduced, by spontaneous dissolution, into very minute particles, each in the *wet* state capable of resuming the regeneration of individual germs. Judging from analogy, it is but reasonable to suppose that it is this reduced nubecular and molecular condition, which adapts them to last and survive in a *dry* condition, as we find it not only with the Fungi, but also in the case of the pruinose-pulverulent, primitive moss-spawns, all three agreeing in this feature of being "reduced to dust," out of which they are again resuscitated. This evanescent condition, however, where gelatinous particles of about $\frac{1}{2000}$ of a line diameter shrink alike to imperceptible dimensions, affords no pretext whatever for assuming identities, just because we ourselves lose the *means of discrimination*. Whenever the identity of *substance* is preserved, each of these various molecular organisms preserves its cyclar developments distinct from similar, corresponding ones as true species, so far as my observations go.

There being, at present, no comprehensive pictorial works available to fall back upon for reference, that are sufficiently correct, even in their designs, to identify the forms, allowance must be made for the liberties of comparison taken in the following descriptive representation of the most frequent infusorial processes.

On extensive clay formations, all over the central part of the Mississippi valley, the first appearance, in the warm season, of vegetable life, *e. g.* in water-pools recently formed by rain, &c., is mainly that of "*Chlamydococcus pluvialis*," even where the clay is immediately taken from the deepest excavations. As the sequel of

my papers will show, this particular form belongs to the common Silver-moss (*Bryum argenteum*) which is widely disseminated all over the surface of the globe, and that, by the way, rather scantily "fructifies" in a "sexual" fashion, *i.e.* by the development of a *theca;* but on clayey soils fills all the sluggish and stagnant waters with its virescent uliginous spawns; while it covers the surface of fields, by millions of acres, with a minute crust, or "*brick-red leprosy*,"* whose fine, molecular dust is swept aloft by every wind. Immediately before the frost, the same fields are densely covered with a small crop of minute moss, doomed to perish in this form, but revived in its spawns at the first thaw in the shape of a universal *chalk-white* "*clay-bloom*," or pruinose efflorescence from the soil, and that in water at once re-develops into the so-called *Protococcus* (or *Chlamydococcus*) *pluvialis* in the form of green flagellate-roving beads.

These minute, but in this instance *coated*, swarming cells are replete with chlorophyll, and are globular ovoid in shape. They have at their smaller end, just where the motor *flagellum* (or vibratory lash) arises, a clear point of substance; wherein, in a small percentage of these cells, *a parasite* is found to develop.

This parasite is a perfectly colourless globule, apparent in the clear navel-point of the cell, and exhibits a faintly opalescent hue. As it grows, the cell which harbours the "incubus" loses its own individual vitality. It ceases to swarm about and dissect into living, chlorophylliferous and automatous progenies, as the live ones do. Instead of spontaneously dissolving as in the living process the cell-coat remains firm; and as the parasitic animal yolk grows and occupies more space, executing tremulous and vibratory contractions, the chlorophyll is pressed into the rear, a lifeless mass. At last the cell is ruptured in front, and the cupular-compressed, dead, chlorophylline mass remains inert and void of life until devoured by Infusoria or the zymotic fungus. The cell-coat, likewise, is effete, while the larger globular and somewhat acicularly-granulated incubus, after a few very wry contractions, at once widely opens a large, ciliate mouth, gaping across the sphere's surface; and disengaging

* See 'St. Louis Med. Reporter,' Jan. 1st, 1867, pp. 522, 527, 528. Also 'Proc. St. Louis Ac. Sc.' (July, 1861), vol. ii., No. 1, p. 160; and vol. i., p. 156. For "Chlorococcum" read "*Sphærocarpus*" (lately renamed "*Protuberans*" Ag.) and its "botrydium" progenies. The latter collapse and turn red. This pulverulent, *miniate* "*Lepraria kermesina*" Auct., must, however, by no means be confounded with the darkly purpureous, uliginous moss-spawns which cover, *e. g.* the hilly "Orange-sand" regions of the State of Mississippi. It is prevalent in winter in damp weather, and consists of matted red "Microcoleus" or lumbricoid (sheathing) moss-cells, each one containing a central brood-fibre which is medullary-dotted, dissecting, and fascicularly surrounded by a stratum of (automatous, prorepent) "Oscillaria"-fibrils. Not only the ultimately enlarged (chlorophylliferous) brood-segments, but also the dark undulating fibre, form brood-balls (terminally). Its gelatine forms a cement of the loose sandy clay, and a home or abode for the Cladoniæ (or bright-green foliolate lichens) as well as for grasses, &c.

or displaying a girdle of cilia round the rear part of the body, it immediately represents the *free-roving Vorticella* in full equipment.

Its subsequent "encystment" into a spherical cyst *densely covered with short prickles* (somewhat like the rim of a Heliopelta) and containing entrail-like designs, is well known.* Also, that it eventually bursts—*occasionally*, at least—and disgorges a peculiar sort of *wafer*-shaped, elliptical (not ellipsoidal!) cells, or nuclei, whose ulterior fate and abode, however, hitherto remained unknown.

The fact is, that they rise to the *surface*, where, on account of their shape, they *inhere*, as an almost imperceptible pellicle or stratum, which to the microscopic observer is the instantaneous index of the precise *focality* of the surface.

In this state, when no food is supplied, they long remain unaltered. When meat, bread, or other nutritive matter is added, however, they *develop* (particularly where the absence of light prevents the confervaceous or green, chlorophylline growths from becoming paramount) into the smallest *Vorticellæ*. The centre of the wafer-shaped disk, inherent in the denser surface of the water, protrudes downward into a little clear knoll or navel, which soon begins to *jerk*, representing as it were a pin-head of $\frac{1}{1000}$ line diameter on a little thread or coiled stalk; and, as the whole increases in size, it now constitutes the well-known *spirally pedunculate* Vorticella; pyriform bell-shaped, somewhat pitcher-mouthed, with cilia around the orifice and a clear granular *nucleus* or "germinal speck" inside.

The multiplication of the pedunculate Vorticellæ, by fission, lengthwise, and by budding-out, sideways, at the rear end, is sufficiently known. As the bud tears loose, there is yet no oral aperture visible. There is, however, a girdle of cilia at the *rear*, wherewith it performs an undulatory vibration, until it tears loose by spinning round; and after irregularly prancing about, and rebounding at headlong speed, within a few minutes it "settles" upon some suitable surface, with the ciliated rear end; and spinning out a *podetium*, or peduncle, often within a quarter of an hour, the rear cilia get applied downward to the body and disappear; while after a little time a fully ciliated mouth now opens *in front*.

As for its *retrograde* multiplications, when a Vorticella gets "sick," for want of air or food (as when kept between glass-plates held apart and glued round about, or cemented, to prevent evaporation), the body contracts to a perfect globe, with a big germinal point, "eye," or nucleus in the middle; the throat contracted, and the lifeless cilia standing around like the limb of a rose-calyx. The germinal speck, ogle, or nucleus soon becomes immensely bloated, protruding out of the crown or ciliate aperture like a dim, hazy *balloon*, and then being either suddenly or gradually exploded, its almost invisible granular contents settle around and increase into a

* In fig. 215, Carp. 'Micr.,' p. 446, the short prickles are omitted, B to E.

rather densely *punctuated cloud*, run up like a *cumulus;* its tips mostly warty as with (dotted) strawberries, a sort of primordial "framboësia." It is to this form that I wish to call particular attention, since it represents *the most minute phase of individual animate life* visible at present under the known powers of the microscope: being the ultimate retrograde development-phase, as well as the first manifestation, common to all such soft primitive *sarcode* bodies, from the "Vorticellan" to the most bulky "Paramecian" forms. And from each little dot in these "clouds of life" a separate Vorticella can be seen to develop! It is here, indeed, at this first *visible* advent or exordium of animate life, and the resurrection of millions of germs through the spontaneous dissolution of a single one, that the last nubecular *microscopic* perceptions closely resemble the last nebular telescopic as well as the *theoretic* ones of Laplace's cosmogony.

For the present let us call such often-repeated forms of retrograde self-dissolution and self-multiplication a germinal *nubecula.* Such likewise occur in a very closely analogous form in the self-maceration (of the engulfed pencil-beads and the enlarged "oidium"-joints) of the yeast or "zymotic" fungus. The difference being, that in the case of *animal* infusoria the dots remain affixed, and, after jerking apart, rapidly travel on separately with a vacillating motion resulting from their warped surface (*true* Zoogloea Termo *Cohn; not* that of *Klob*), while in the case of the mucous or polypoid fungine diffluence (erroneously called by the above name by *Klob*), the component blunt-cylindrical Cacteria are not mutually fixed, but all simultaneously slowly move onward and apart in slimy *trails*, without a perceptible rotation, simply quivering, and ultimately enlarge into fibres. Of the *black* or "indissoluble" nebula-form of the zymotic or yeast-fungus I have already treated in the 'St. Louis Medical Reporter,' Jan. 1st, 1868 (Zymotic Condition, &c.), and 'Proc. A. A. A. S.,' 1870, *et ante.* Neither the animal molecules nor the (coated) "Cacteria" join into file, as do the fermentic sarcode-vibrios, being *naked.*

The watchful observer will have opportunity of witnessing another sort of "fertile dissolution" of the more bulky Vorticellæ, by the discharge of certain globular, granular "pellets," surrounded with other adhering and *jerking* particles—(probably the "acineta" of some authors)—while containing the "currant-shaped" yolks,—hereafter to be described, together with its "amœba" or pseudopodial dissolution, when treating of the so-called "*Paramecium Aurelia.*"

A direct *onward* evolution of Vorticella I had occasion to realize on the fetid scum cuticle of a putrescent aquarium. All the Vorticellæ which, in dense clusters, lined the under surface of that membrane, or animal pellicle, were found to *elongate* into a sort of

roughish, but very hyaline, cucumber-shaped form; each "cucumber" at first crowned with a true vorticellan pitcher-mouth, mostly, however, closed and rounded over, occasionally gaping or as it were *yawning* spasmodically, at intervals only, and which finally "shut up" for good. The little glassy knobs—like so many trunk-nails—that covered the surface, grow into soft jerking-bristles;* the mouth into the well-known mustachioed slit of barbiform *cilia;* and the wan, limpid, empty and now entirely flattened, ligulate or sandal-shaped body tears loose as a young fluttering, pallescent *Oxytricha* (*Pelionella*), or so-called "hackle-animalcule"; *darting* by the jerks of its stiffish marginal bristles, and by the constant "plying" of the long-barbed, ciliate slit effecting its slower progress. It never revolves, but often crawls; both in contradistinction to the fleeced, revolving, and vacuole-propelled "Paramecium" form.

This is probably the "short-line" development of Oxytricha, directly from the germinal clouds or the parasite of *Chlamydococcus* through Vorticella. I have no good figures to refer to, since even the detailed ones of Ehrenberg, in 'Trans. Berlin Acad. Sc.,' 1833, tab. iii., figs. ii., iii., and iv., which belong here (as well as tab. xxiv. and xxv. of A. Pritchard's 'Hist. Inf.,' 1861), are too inaccurate to serve as a guide, or to be readily identifiable even by those acquainted with the real natural object-material itself.

At first the bristles of the (tongue-shaped, flat, and elongate-elliptical) Oxytricha are fluttering and tremulous; but as it feeds and rapidly increases in bulk, all the well-known characters of the complete "Oxytricha," its stiffish darting-bristles, its grumose, obscured body, irregularly replete with granular yolks, and very frequently cross-dividing, become typified. When thus cross-divided (a process well known and abundantly figured) the front part alone retains the barbed mouth, which, from the apex, switches down like a mustachio on a longitudinal slit about a quarter of the whole length. The blunt rear-part, on the contrary, separates with an incurrent angle which soon contracts into a new mouth, whereby the rear animal takes a blunter shape (like the cotyledon of an almond, the flat side downward).

The Oxytricha is by no means, however, to be considered as an adult form, since it is never seen to exhibit a continuity as of a membrane, or of internal ducts or viscera. Neither is it seen to "copulate" or adhere lengthwise, or in any other fashion to one another except in the process of self-division. Nor does it readily divide by longitudinal fission in this state. I have seen this only

* In their onward development these softish bristles are indurated into "styles" (of specialists). The attentive reader will observe that on such alterations of growth alone a great many colloiding false genera and species have been formed. *Sap. sat.* We have naturally to reject all, of which the *mode* of development remains unknown, as indicating a false stand-point.

once. Crawling and darting by an apparatus of marginal bristles, prolonged in front and particularly in the rear, it is destitute of the "propulsive vacuoles," as found, *e. g.* in the large "*Paramecium Aurelia*;" but besides being studded, particularly in the rear portion, with a great number of larger and smaller granular *pellets*, its body exhibits near the middle a large, clear, and granular "germinal speck" or *nucleus*, which is often observed to swell, protruding globularly over the surface, below and above, when seen crawling in profile.*

Occasionally it is seen to extrude suddenly that turgid germinal nucleus or yolk (*vitellus*), which, as in all these cases, is itself coatless, but hung around with divers jerking molecular fragments torn loose from the parental body, which is *ruptured on the spot*, but readily "re-cemented," as it were.

The larger of these coatless granular yolks (constituting the original *pseudo*-genus, and species "*Zooglœa Termo* Dujard") † mostly consist of two parts, *viz.* a general "albumen" of a granular and evidently *trabecular* texture, enclosing one or two *distinctly coated*, quite hyaline and perfectly *globular vesicles*. The latter resemble in shape a very clear white currant, as it were, by having a sharply-defined circlet inscribed near one side, that is caused by a local *inversion* of contents (somewhat like the air-vesicle within a hen's egg).

(*To be continued.*)

* The transformation of the "rear-part" of Oxytricha, as given by J. Haime in 'Ann. Sci. Nat.,' Ser. 3, tom. xix., p. 109 (and represented in Carp. 'Micr.,' pp. 447 and 448), I have not been able to verify myself; it must not, however, by any means be confounded with the *encystments* (1) of Vorticella, producing wafer-like molecules; (2) of the non-pulsating, tear-shaped "Paramecium kolpoda" grub, producing free Oxytricha; nor (3) with that of the "oyster" or "porte-monnaie" grub, producing paramecium-like bodies, Gregarina-fashion; nor either (4), with some large Oxytricha "currants," containing the revolving "crucible."

† As represented by Cohn in 'Nov. Act. Nat. Curios,' 1854, vol. i., tab. xv., fig. ix. In Klob's 'Microscopic Researches on Cholera' the term is misapplied to engorged joints of dissected corruptive fibrils (or "Oidium lactis") replete with bacterial daughter-cells.

PROGRESS OF MICROSCOPICAL SCIENCE.

Difficulty of Experiments on Spontaneous Generation.—Mr. Crace-Calvert, F.R.S., cites the following experiment in his first paper "On Protoplasmic Life," read before the Royal Society (received May 8):—Although he was prepared, by the perusal of the papers of many workers in this field, to experience difficulties in prosecuting the study, he confesses he did not calculate on encountering so many as he met, and especially those arising from the rapid development of germ-life, and of which he had hitherto seen no notice in any papers which had come under his observation. Thus, if the white of a new-laid egg be mixed with water (free from life), and exposed to the atmosphere for only fifteen minutes, in the months of August or September, it will show life in abundance. From this cause he was misled in many of his earlier experiments, not having been sufficiently careful to avoid even momentary exposure of the fluids to the atmosphere. To the want of the knowledge of this fact may be traced the erroneous conclusions arrived at by several gentlemen who had devoted their attention to the subject of spontaneous generation.

Regeneration of the Corneal Epithelium.—Dr. Hjalmar Heiberg, of Copenhagen, has written a very valuable paper on the above subject which is reproduced in the 'Lancet.' A paper recently appeared in Virchow's 'Archiv,' by Julius Arnold, on the same subject, who came to the conclusion that the new cells which replaced the old, when these had been detached, were derived from a finely granular blastema that changes into protoplasm, and that in this protoplasm the new cells arise by a process of free cell-formation. The correctness of this conclusion is contested by Dr. Heiberg, who maintains the view that young cells are developed from the old, in which certain changes have taken place. His mode of procedure was to scratch the surface of the cornea with a cataract needle in animals (frogs, birds, rats), and, after the lapse of from eighteen to forty hours, to remove the eye and examine the cornea both by means of fresh sections and after careful preparation in solutions of chloride of gold (maceration for from three to five minutes in a one-half per cent. solution of the salt). In certain preliminary experiments it was found that the injured part *immediately* after the injury presented sharply-defined irregular borders; after six hours the margins were considerably flattened, so that the boundary of the abrasion was much less distinct. After eighteen hours it was difficult to tell the seat of the injury with the naked eye, and its diameter had become reduced to one-half or one-third; and after forty hours recovery was complete. He convinced himself by microscopic investigation that the process of regeneration of the epithelium proceeds from the margins of the abrasion, the layers of cells immediately bounding the seat of injury becoming elongated, and, as it were, sending forth processes towards its centre; so that the margins are rendered very oblique, whilst at the same time the exposed surface of the cornea is raised considerably above the level of that which is still covered by the cells. Sometimes the cell-processes become detached and contract

into glassy, clear, rounded masses; but he has never noticed the free formation of cells in a blastema, and does not believe that the white corpuscles of the blood or "migrating cells" play any part in the regeneration of the cells, though he has occasionally met with them between the epithelial cells and in the substance of the cornea in the vicinity of the injury. In no single instance has he observed any appearance leading him to think that they undergo conversion into epithelial cells. The processes thrust forth by the cells, as above mentioned, were found, from observations extending over many hours, during which the cornea was placed in a current of serum, to undergo slow changes of form and size, the movements however being in no way comparable in activity to those of amœboid cells. The cells situated at a greater distance from the injured part often became granular, and their margins more distinct. In rats both the upper flattened and the deeper, more columnar cells seemed alike to thrust forth the processes destined to cover the floor of the injured part. Dr. Heiberg expressly remarks that he has not satisfactorily followed the separation of the cell-processes to form new cells, which again push out new outgrowths, to be again detached, though he obviously thinks this is what really occurs. Dr. Heiberg gives several drawings to illustrate the points mentioned in his paper, which is in every respect a most valuable contribution.

The Microscopic Structure of Cotton-seeds.—This is a paper, published in the 'Neues Jahrbuch für Pharmacie,'* by Dr. F. Flückiger. It is a long paper, and contains, in the first place, a succinctly-written botanical description of the several kinds of shrubs and trees from which cotton is obtained; next, we meet with a histological and morphological minute description of the microscopical structure of the cotton-seeds, illustrated by coloured lithographs. The latter portion of this essay is devoted to a well-condensed and very complete review of all that has been done in various parts of the world in reference to the scientific, as well as industrial, researches on cotton-seeds, and the oil it yields. According to the author, the quantity of cotton-seed annually gathered amounts to 1,000,000,000 of kilos., which, under the least favourable conditions, would yield 150,000,000 kilos. of oil.

Discovery of the Animal of the Spongiadæ confirmed.—Mr. H. J. Carter, F.R.S., sends a line to 'Silliman's American Journal' (July), "just to tell you what you will be glad to learn, *viz.* that I have confirmed all that Professor James Clark, of Boston, has stated about the sponge-cell, and much more too. It is after all only what was published and illustrated in the 'Annals' in 1857. Indeed I am astonished now at the accuracy and detail of that paper,† now *all* confirmed by an examination of a *marine* calcareous sponge. I have not only fed the sponge with indigo, and examined all at the moment, but the sponge so fed was put into spirit directly afterward, and *now* shows all the cells (monociliated) with the *cilium attached and the indigo still in the cells.* This, I think, will break down Häckel's hypothesis, which is as imaginative and incorrect as it is beautiful. His "Magosphæra" too is figured in the 'Annals' (1856), and de-

* Double number, May and June, 1871.

† "Ultimate Structure of Spongilla," &c.

scribed *in extenso* as the amœboid cell which inhabits the mucus of the cells or internodes of the Bombay great *Nitella*. But there are no people in England, if on the Continent, who seem to be able to show this, if even they be cognizant of it. *Ex oriente lux* used to be the old phrase; the light is now being *reflected* back from America. It is from there that we must expect novelties now."

The Anatomy of the Graafian Follicles in Man.—These have been very fully explained by Dr. Kronid Slavjansky in Virchow's 'Archiv,'* and are well abstracted by the 'Lancet' in a late leader. The author's observations have been made on subjects of all ages brought to the Anatomical and Pathological Institute of the Medico-Chirurgical Academy of St. Petersburgh. The preparations were macerated immediately after removal from the body (which was usually effected in from three to eight hours after death) for a week or a fortnight in Muller's fluid, then placed in a 70 per cent. solution of spirits of wine, and then in a 90 per cent., after which fine sections were made. In the examination of the ovaries of children he recognizes three forms of follicles —(1) primordial follicles, which are the youngest; (2) a transitional form to the mature follicles; and (3) more or less developed mature Graafian follicles.

The primordial follicles are of roundish or elliptic form. They consist of the primordial egg, with the yelk, the germinal vesicle, and germinal spot surrounded by a series of small round cells in contact with one another, and without any intermediate substance. In very young follicles a spot can always be found where these cell-series are interrupted, and where the egg apparently is in direct contact with the wall of the follicle. This is the follicle-pole of Pflüger. At a subsequent period this spot is covered by a layer of cells. The wall of the follicle presents no characters distinguishing it from other parts of the ordinary stroma of the ovary. On its inner surface, however, he admits the presence of a lustrous layer, similar to that found in acinous glands, in which sometimes a few fusiform cells may be discovered, and thus differs from Waldeyer, who in his description of the structure of the primordial follicles states that they are destitute of a membrana propria, and that the stroma tissue lies in immediate contact with the contents of the follicle, the fibres being arranged circularly around it. Thus it appears that in the very youngest follicles the egg is surrounded by a series of epithelial cells representing the future membrana granulosa. The delicacy and instability of these cells explain the circumstance of their being denied by Klebs, Schron, and Florinsky. Such follicles as those that have just been described may be seen in certain parts of the ovary, at every age from the eleventh week of intra-uterine life to the grand climacteric. In embryos they are usually found between the cortical and medullary layers; in newborn children they are found in all parts; in adults they are met with only in the most superficial layers of the cortex, where they are closely packed, and when badly prepared, in consequence of the destruction of the epithelium, look like rows of large cells from which, near the centre of the ovary, the follicles appear to develop. Slavjansky accepts and corroborates Waldeyer's description of the origin

* Band 51, Heft. 4.

of the primordial follicles from tubes. He describes a somewhat complicated histological process in which the follicles undergo a physiological destruction; this commences with a fatty degeneration of the follicular walls, and proceeds with contraction of the cavity till it is completely obliterated. The young tissue by which this obliteration is effected is considered by M. Slavjansky to proceed from the white corpuscles of the blood. In regard to the formation of the corpus luteum (verum and spurium) he concludes that in this process only the so-called granulation layer of the follicular wall is active, the membrana granulosa undergoing fatty degeneration and atrophy. In conclusion, he describes the pathological conditions of the ovary, with the exception of dropsy of the Graafian follicles. He divides them into affections of the parenchyma, and affections of the wall of the follicle. The former are included under the heads of fatty metamorphosis and colloid metamorphosis; the latter he considers may be embraced under the general head of sclerosis.

The Structure of Stigmaria has been most fully gone into in a paper by Professor Williamson, F.R.S. (of St. John's College) in the last number of the 'Proceedings of the Royal Society.' *Stigmaria* is shown to have been much misunderstood, so far as the details of its structure are concerned, especially of late years. In his memoir of *Sigillaria elegans*, published in 1839, M. Brongniart gave a description of it, which, though limited to a small portion of its structure, was, as far as it went, a remarkably correct one. The plant now well known to be a root of *Sigillaria*, possessed a cellular pith without any trace of a distinct outer zone of medullary vessels, such as is universal amongst the *Lepidodendra*. The pith is immediately surrounded by a thick and well-developed ligneous cylinder, which contains two distinct sets of primary and secondary medullary rays. The primary ones are of large size, and are arranged in regular quincunctial order; they are composed of thick masses of mural cellular tissue. A tangential section of each ray exhibits a lenticular outline, the long axis of which corresponds with that of the stem. These rays pass directly outwards from pith to bark, and separate the larger woody wedges which constitute so distinct a feature in all transverse sections of this zone, and each of which consists of aggregated laminæ of barred vessels disposed in very regular radiating series. The smaller rays consist of vertical piles of cells, arranged in single rows, and often consisting of but one, two, or three cells in each vertical series; these latter are very numerous, and intervene between all the numerous radiating laminæ of vessels that constitute the larger wedges of woody tissue. The vessels going to the rootlets are not given off from the pith, as Goeppert supposed, but from the sides of the woody wedges bounding the *upper* part of the several large lenticular medullary rays, those of the *lower* portion of the ray taking no part in the constitution of the vascular bundles. The vessels of the region in question descend vertically and parallel to each other until they come in contact with the medullary ray, when they are suddenly deflected, in large numbers, in an outward direction, and nearly at right angles to their previous course, to reach the rootlets. But only a small number reach their destination, the great majority of the deflected vessels terminating

in the woody zone. A very thick bark surrounds the woody zone. Immediately in contact with the latter it consists of a thin layer of delicate vertically elongated cellular tissue, in which the mural tissues of the outer extremities of the medullary rays become merged. Externally to this structure is a thick parenchyma, which quickly assumes a more or less prosenchymatous form and becomes arranged in thin radiating laminæ as it extends outwards. The epidermal layer consists of cellular parenchyma with vertically elongated cells at its inner surface, which feebly represents the bast-layer of the other forms of Lepidodendroid plants. The rootlets consist of an outer layer of parenchyma, derived from the epidermal parenchyma. Within this is a cylindrical space, the tissue of which has always disappeared. In the centre is a bundle of vessels surrounded by a cylinder of very delicate cellular tissue, prolonged either from one of the medullary rays or from the delicate innermost layer of the bark, because it always accompanies the vessels in their progress outwards through the middle and outer barks. It is, he says, evident that all these Lepidodendroid and Sigillarian plants must be included in one common family, and that the separation of the latter from the former as a group of Gymnosperms, as suggested by M. Brongniart, must be abandoned. The remarkable development of exogenous woody structures in most members of the entire family indicates the necessity of ceasing to apply either to them or to their living representatives the term Acrogenous. Hence the author proposes a division of the vascular Cryptogams into an exogenous group, containing *Lycopodiaceæ*, *Equisetaceæ*, and the fossil *Calamitaceæ*, and an endogenous group, containing the ferns; the former uniting the Cryptogams with the Exogens through the *Cycadeæ* and other Gymnosperms, and the latter linking them with the Endogens through the *Palmaceæ*.

Passage of Corpuscles through the Blood-vessels. — A long and important paper on this subject has been lately read before the Royal Society, by Dr. R. Norris, Professor of Physiology in Queen's College, Birmingham. He says that on a careful consideration of the hypotheses which have been propounded by Waller, Cohnheim, Stricker, Bastian, and Caton, to account for the curious phenomena in question, it will be found that all these hypotheses fall short in one important particular, inasmuch as they afford no explanation whatever of by far the most singular part of the process, *viz.* the fact that the apertures through which the corpuscles pass again close up and become invisible. The question, indeed, is not so much how the corpuscles get out, as how they get out without leaving any permanent trace of the apertures through which they have so recently passed, and which were so palpable during the period of transit. Before proceeding to elaborate his own views, he restates succinctly the various points upon which observers are agreed. 1st. Both white and red corpuscles pass out of the vessels through apertures which can neither be seen before their ingress into or egress from the vessel wall, but only during the period of transit. 2nd. An essential and primary step in the process is, that the corpuscles shall adhere or, more properly, cohere to the wall of the vessel. 3rd. These cohering corpuscles shall subsequently be subjected to pressure from within.

From this he proceeds to consider the various physical conditions involved in the question, and eventually he states that all that is essential for a rigid or plastic body to pass through a colloid film is:—1st, an intimate power of cohesion, either mediately or immediately, between the film and the body; 2nd, a certain amount of pressure from within; 3rd, power in the substance of the film to cohere to the surface of the body (or to some intermediate matter which already coheres to the surface) during its passage; 4th, cohesive plasticity of the particles of the material of which the film itself is composed, so that the breach in it may again become reunited as it descends upon the opposite surface of the body which is being extruded. These conditions he thinks are prevalent in the case of the minute blood-vessel and the corpuscle; since he considers that the passage of the corpuscle through the vessel is precisely analogous to the passage of a body through a soap bubble. Unfortunately in parts the author's language is not sufficiently clear to enable one to find his exact meaning, but we believe that we have found it correctly as above.

The Structure of Lepidodendron is stated by Professor Williamson to be in the case of *Lepidodendron selaginoides* as follows:—It consists of a central medullary axis composed of a combination of transversely barred vessels with similarly barred cells; the vessels are arranged without any special linear order. This tissue is closely surrounded by a second and narrow ring, also of barred vessels, but of smaller size, and arranged in vertical laminæ which radiate from within outwards. These laminæ are separated by short vertical piles of cells, believed to be medullary rays. In the transverse section the intersected mouths of the vessels form radiating lines, and the whole structure is regarded as an early type of an exogenous cylinder; it is from this cylinder alone that the vascular bundles going to the leaves are given off. This woody zone is surrounded by a very thick cortical layer, which is parenchymatous at its inner part, the cells being without definite order; but externally they become prosenchymatous, and are arranged in radiating lines, which latter tendency is observed to manifest itself whenever the bark-cells assume the prosenchymatous type. Outside the bark is an epidermal layer, separated from the rest of the bark by a thin bast-layer of prosenchyma, the cells of which are developed into a tubular and almost vascular form; but the vessels are never barred, being essentially of the fibrous type. Externally to this bast-layer is a more superficial epiderm of parenchyma, supporting the bases of leaves, which consist of similar parenchymatous tissue. Tangential sections of these outer cortical tissues show that the so-called "decorticated" specimens of *Lepidodendra* and of other allied plants are merely examples that have lost their epidermal layer or had it converted into coal, this layer, strengthened by the bast tissue of its inner surface, having remained as a hollow cylinder when all the more internal structures had been destroyed or removed.—*Proceedings of Royal Society*, vol. xix., No. 129.

NOTES AND MEMORANDA.

The Darwinian Question. A Prize Essay.—It seems that by the provisions of the late Dr. William J. Walker's foundation two prizes are annually offered by the Boston Society of Natural History for the best memoirs, written in the English language, on subjects proposed by a committee appointed by the Council. For the best memoir presented, a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased, at the discretion of the committee, to one hundred dollars. For the memoir next in value, a sum not exceeding fifty dollars may be given; but neither of these prizes are to be awarded unless the papers under consideration are deemed of adequate merits.

Memoirs offered in competition for these prizes must be forwarded on or before April 1st, 1872, prepaid and addressed

"*Boston Society of Natural History,*
for the Committee on the Walker Prizes,
Boston, Mass."

Each memoir must be accompanied by a sealed envelope enclosing the author's name, and superscribed by a motto corresponding to one borne by the manuscript.

Subject of the Annual Prize for 1872.—"The Darwinian Question; its bearings on the development of animal life."

The Application of Mr. Darwin's Theory to Flowers and the insects which visit them, by Dr. Erm Müller, with notes by Prof. F. Delpino, is now published in English. It has been translated from the Italian by an American naturalist, and may be had for 25 cents by addressing to the Naturalists' Agency, Salem, Massachusetts.

Dr. Dudgeon's Microscope.—A writer in the 'American Naturalist' for September, comments on this instrument: he says that while the common "tank microscope" can be worked best somewhat horizontally, through the side of the tank, this arrangement, besides being applicable to much higher powers, is adapted to give a more or less vertical view, being entirely free from any tremor on account of the motion of the top of the water, and is therefore especially useful for dissecting purposes. Its object, though not its method, is identical with that of Tolles' immersion objective for low powers, published more than two years ago; though the latter naturally possesses—being constructed especially for this use, and dispensing with two unnecessary surfaces of glass—some optical superiority, as well as a much longer working focus. The submersion tube, being applicable to ordinary lenses, only slightly lowering their magnifying power and considerably shortening their working focus, will doubtless be extensively useful; though the statement that it may be always retained in position as a protecting cover to the lens without impairing the definition or illumination in ordinary work, must be considered as too enthusiastic. It is especially applicable to lenses of from one inch to one-quarter inch

focus (the latter limited to a very small angle), and the objects should be placed in a jar or tank having the bottom and at least one side quite smooth and transparent.

How to study Embryology.—This is well shown by the recent observations of Dr. Elias Metschnikoff upon the development of scorpions. This author has recently published in Siebold and Kölliker's 'Zeitschrift,' a paper which is abstracted by the 'American Naturalist.' The subject is the embryology of the *Scorpio Italicus* and of a species from Tyrol. The embryology of insects and crustacea as pursued at the present day by zoologists, who are directing especial attention to the provisional membranes of the egg and embryo, depends almost as much on the skilful use of the chemicals as the microscope itself. The author says "the methods which I employ in these researches are not complicated. I study the eggs removed from the ovarian tubes; or, place the living embryo in a drop of a weak solution of salt (salzlösung); or I at first submit them to the influence of solutions of chromic acid of different strengths, and then examine them either with a simple or compound microscope. Out of embryos hardened in this way I can make sections. Much of the time I have to work with dissecting needles, while the embryos or portions of them treated in this way, and in an equal mixture of fresh and salt water, afford very good objects for study." The embryology of scorpions was sketched out in a general way by the distinguished German embryologist Rathke. Metschnikoff extends these researches very greatly, and considers as the most important results of his studies the discovery that "in the embryo of the scorpions three embryonal membranes are developed, which in many respects are very strikingly similar to the Remakian embryonal membranes of the vertebrates."

CORRESPONDENCE.

What is the Aperture of Tolles' 1/5th?

To the Editor of the 'Monthly Microscopical Journal.'

Sept. 18th.

Sir,—In the very interesting account given by Colonel Woodward of Mr. Tolles' fifth there is one important omission. He has not told us what is its aperture. As a general rule a one-fifth by any of the best of our English makers does not exceed 100° in aperture; but it would not "surprise me to hear" that Mr. Tolles' fifth has an aperture of 150° or 160°. At the close of his paper it is suggested that trials should be made with recent English glasses of the same focus, so as to compare the results with those obtained with Mr. Tolles'; but until the respective apertures are known, such comparisons would be without meaning. Perhaps Colonel Woodward, if this should meet his eye, will be kind enough to supply the omission.

As I am writing on the subject, I may take the opportunity to point out more generally the confusion which exists in the minds of

most microscopists about the classification of glasses. It so happens, and it is a convenient arrangement, that they are named from their focal lengths. The focus, therefore, has come to be considered the *essence* of the glass, the aperture being counted only one of its accidents or attributes; like, for example, good or bad workmanship. And so, for comparison of performance, we see objectives tried against others of the same focus only. But there is, scientifically, no reason whatever why glasses should not receive their rank or denomination from their apertures instead of their foci. Thus, for example, a glass of 90° with half-inch focus may be compared with a glass of 90° quarter-inch focus with just as much fairness as against another half-inch of 45°. Not only so, but it is even more reasonable, scientifically, to compare by apertures than by foci, for the want of amplification may be not ill compensated by the action of the eye-piece; whereas by no subsequent manipulation of the eye-piece can the peculiar qualities depending on difference of angle ever be balanced. Focus and aperture are in fact both essential factors in the denomination of an object-glass, and where a difference exists in either we must keep in mind that we are comparing different things and not the same things with differing qualities. A curious illustration of the confusion I have commented on may be seen in a pamphlet or little book about microscopes, rather widely circulated, I believe, some years ago—a reprint from a paper read before the Royal Microscopical Society. The author in a learned note undertakes to prove that objectives with wide apertures are the best; his reason being that with a half-inch of 90° he can see things which he cannot see with a half-inch of 55°. It does not seem to have occurred to him that, if this be all, an eighth of an inch will show him more than either of them; and therefore with equal wisdom of logic he might have drawn the conclusion that an eighth is preferable to a half-inch.

It is customary in your Journal to sign with the writer's name; but as the purpose of this letter is only to ask a question, it will do equally well, when asked, by a letter of the alphabet.

Yours, &c.,

B.

To the Editor of the 'Monthly Microscopical Journal.'

October 20, 1871.

Dear Sir,—Will you permit me through your columns to say, in answer to inquiries as to where Dr. Sanderson's paper "On the Pathology of Contagion" appears, and to which I refer in my observations on the "Fungoid Origin of Disease," page 156, September number of the Journal, that it is part of an Appendix to the Thirteenth Annual Report of the Medical Officer of the Privy Council, published by Hansard, Great Queen Street, for the small sum of fourpence-halfpenny.

Yours very faithfully,

Jabez Hogg.

Dr. Lawson,
Editor 'Microscopical Journal,' &c., &c.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *Oct.* 4, 1871.

Wm. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors. Among the donations was a series of preparations of intestinal worms from Australia, which Dr. Cobbold had been requested by the Secretaries to report upon.

To the President of the Royal Microscopical Society, London.

137, Castlereagh Street, Sydney, *July* 12, 1871.

Sir,—By this mail I have sent to your Society twenty-four micro-specimens prepared by me, in order that your Society may identify and name the several entozoa hereby forwarded.

Our Agricultural Society have also sent you a copy of their journal, in which you will see that I have given a description of the lung, intestinal, and tape worm infecting our sheep, with illustrations of the principal characteristic features of said worms. In looking over the illustrations, please make a note of × 35 instead of × 25 (an inch objective being used with the camera lucida).

At the present time our cattle, sheep, and pigs, are all infested with parasites, and within the last few days another new worm has been sent me to investigate, its habitat being in the fat (or flip) surrounding the kidney of the pig. I have sent you six different preparations: a male and female; caudal extremity of the male, showing cup-like bursa with a double (V-shaped) spiculum; head, showing oral orifice with six papillæ leading into the œsophagus—the papillæ are armed with minute lancet-pointed teeth; also two preparations showing the different stages of the development of the eggs.

I have also sent you what I take to be an "*Amphistoma conicum*" from the rumen of the sheep. Mr. Krefft (our curator of the Museum) says it is a larva of a distoma. I maintain that it is a sexually mature animal, and have named it "*A. conicum.*" Your opinion on it will be received with thanks.

I have also sent you four preparations of a "shrips" which has attacked the whole of our deciduous trees and shrubs, causing the leaves to assume a brownish colour. The segments of its body do not correspond to the shrips I have been accustomed to examine. I have isolated one of its wings, and prepared it for your inspection: no doubt of its being a "Thysanoptera," but I have declined to name it.

No. 24 is a pretty little aphis (larva) from the gum (Eucalypti) leaf; it is generally found between two leaves glued together. I do not know its name, therefore would feel obliged if you could name it for me. All the other preparations you will find described in the

* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

journal sent you by our Society, therefore I shall devote the rest of my remarks to the pig-worm.

This worm when seen in the fat surrounding the kidney is of a greyish-brown colour, with a red head (the colour of the head is caused by the fresh blood). It is about 1½ inch in length, the male being somewhat less than the female. It is found in the fat in a free and encysted state, the encysted state being its final stage of existence. When once it becomes encysted the solid parts of the worm ultimately disappear, leaving a greyish-brown fluid containing thousands of eggs. No. 17 are the eggs when the worms (generally from three to six found in a cyst) are beginning to break up. No. 18 are the eggs when the worms have entirely disappeared. The cyst from which this specimen was taken was of about 1¾ of an inch in length and ¼ an inch diameter. By carefully examining this specimen you will see that the contents of the egg-case are already taking on the characteristic features of young worms. Probably in a few days more these young worms would soon have been so far developed as to undertake an independent existence in the flesh of their host. As several pigs have died lately from some mysterious disease, it is possible that this worm or its brood may be the cause, and I think I shall be put in possession of facts which in a few days may clear up the whole history of this parasite. If physiologists had not settled the Trichinæ theory, I think this worm would have played an important part in giving conclusive evidence as to how the worms might get into the flesh of an animal. Here we have got a worm already in the flesh of the animal, it becomes encysted, leaving its eggs to be hatched; when hatched where do they go to? evidently into or through the substance of the body of their host.

It is just possible that some pigs may survive the irritation such a swarm of young worms must set up; others again may die from peritonitis, hence the sudden deaths amongst the pigs. In the Trichinæ theory as at present accepted by physiologists, an animal must eat the meat containing the encapsuled trichinæ. In twenty-four hours the capsules burst, liberating the young trichinæ. In forty-eight hours the young trichina has become a sexually mature worm *containing eggs* and *young embryos*. Then these young trichinæ must pass through the walls of the intestinal tract to get to the muscle and then become encysted. Which of these two cases is the most feasible?

Examine this female worm, you will find its intestinal and uterine canals looped up into a series of convolutions. The trichina has the same characteristic feature. However, I merely throw out the hint to induce others to investigate, and not be content with the opinion of compilers.

Trusting you will excuse this freedom I have taken in asking you to identify the various specimens I have induced our Society to forward you,

I am, yours very truly,

WM. MORRIS, L.F.P.G.S.

P.S.—I am a subscriber to your 'Monthly Microscopical Journal,' and also to the 'Quarterly Microscopical Journal.'

W. M.

Report on Dr. Morris' Paper.

The Secretary of the Society having done me the honour to submit Mr. Morris' excellent series of preparations to my scrutiny, I have to state that the nineteen slides of entozoa comprise six distinct species of parasites. These are severally referable to the *Tænia expansa*, *Strongylus filaria*, *Strongylus contortus*, *Dochmius hypostomus*, *Stephanurus dentatus*, and lastly, as the author correctly surmised, *Amphistoma conicum*.

Though not one of these helminths can be said to be entirely new to science, yet all of them are of considerable interest, and much remains to be worked out in reference to their developmental history.

By far the most interesting parasite is the form which I have unhesitatingly referred to the hitherto little-known *Stephanurus*, first discovered by Natterer, some thirty-five years since, in Brazil. This helminth has recently been found abundantly in the pigs of the United States, where it has been generally regarded as an entirely new species of entozoon. Professor Verrill, of Yale College, Connecticut, has thus been led to describe it under the new name of *Sclerostoma pinguicola*; but the specific and generic titles assigned to the worm by Diesing, in the 'Annalen des Wiener Museums' for 1839, must, of course, be allowed to hold priority.

In the pages of the 'British Medical Journal' for Jan. 14th of the present year (pp. 50, 51), I first announced the true history of this supposed new worm, from an examination of specimens sent to me for identification by Professor W. B. Fletcher, of the Indiana Medical College, U.S.; and it is therefore extremely interesting to me to find, from an inspection of Mr. Morris' contributions to the Society, that the *Stephanurus dentatus* likewise infests the pigs which are reared in Australia.

(Signed) T. S. Cobbold, M.D., F.R.S.

To the President of the Royal Microscopical Society, London.

Sydney, N. S. Wales, *July* 11, 1871.

Sir,—I am instructed by the Council of the Agricultural Society of New South Wales to forward by this mail a small case containing micro-specimens of intestinal worms and insects, which Dr. Morris, L.F.P.G.S., has submitted to our Scientific Committee, with a view of obtaining information on a subject now becoming of great moment for all those who are interested in farming and pastoral pursuits.

Our Council will feel obliged to you if you will have the kindness to lay these specimens before your Society, and furnish us with the result of your deliberations on the matter.

I have the honour to be, Sir,

Your obedient servant,

Jules Joulust,
Secretary.

A List of Twenty-four Micro-specimens of Intestinal Worms and Insects, sent by W. Morris, L.F.P.G.S., sent through the Agricultural Society of New South Wales, Sydney, to the Royal Microscopical Society, London, for identification.

Nos.	Classification.
1	Head of tape-worm.
2	Segment of tape-worm.
3	Ditto ditto, further developed.
4	Female lung-worm.
5	Ditto ditto, prepared to show eggs and embryo worms *in situ*.
6	Male lung-worm.
7	Embryo worms in mucus from the bronchi.
8	Female intestinal worm from third stomach of the sheep, prepared to show spiral canals, eggs, &c.
9	Female intestinal worm (natural state), showing barb-like appendages near the head.
10	Male intestinal worm.
11	Female intestinal worm found in the fœcal matter in the rectum of the sheep.
12	Male ditto ditto. The sheep in which these two worms were got, Nos. 8 and 9 were not found, but a very large number of the Amphistoma were seen in the rumen of said sheep.
13	Female worm found in the fat surrounding the kidney of the pig.
14	Male ditto ditto.
15	Head, the worm showing oral orifice with the papillæ armed with minute lancet-pointed teeth.
16	Caudal extremity of the male.
17	Eggs of the pig-worm.
18	Ditto ditto, taken from a cyst $\frac{3}{4}$ in. in length and $\frac{1}{2}$ in. in diameter, the solid parts of the worm having disappeared, leaving a brownish fluid in the cyst, this, No. 18, being a specimen of said fluid.
19	*Amphistoma conicum*, this worm being disputed by Mr. Krefft, curator of the Museum, as to its being an Amphistoma, he stating it to be a larva of a distoma, I maintaining that it is a sexual mature worm on account of its eggs, &c.
In various stages of development. 20	Small insect from the Isabella vine. This fly has attacked the whole of our deciduous trees, swarming in millions upon the leaves.
21	Ditto ditto, embryo wings.
22	Ditto ditto, without wings.
23	Wing of insect showing it is a Thysanoptera, but we decline to name it.
24	Aphis (larva) from the leaf of the Eucalyptus, found generally between two leaves, gummed together.

Mr. McIntire contributed a paper entitled "An Incident in the Life of a Chelifer."

Dr. Cobbold then proceeded to give an extended comment upon his Report on the Specimens of Intestinal Worms from Australia. Passing in rapid review the various species which were well known in this country, and therefore needed no description, he came to the less known forms, one of which (*Stephanurus*) was first discovered by Natterer, the traveller, on one of the Brazilian rivers (the Barra do, Rio Negro), on the 24th March, 1834. Since then no one appeared to have seen this parasite, or if seen, it had not been recognized. Thirty-

seven years had thus passed without the parasite being discovered. Recently, however, he had received a letter from Prof. Fletcher, of Indiana, describing this very animal; and in a second communication from that gentleman, he hazarded the opinion that this parasite was the cause of the cholera, which had created such havoc in the pork-producing parts of America within the last ten years. The special interest attaching to this statement of Prof. Fletcher was, that in all probability the parasite, though so large (the male being one inch, and the female an inch and a half long), had been overlooked hitherto. Now it so happened that in the communication from Mr. Morris, which the Secretaries had placed in his (Dr. C.'s) hands, the opinion was also given that *Stephanurus* was the cause of the mysterious disease now rife in Australia. Pathologically, another point of interest was that, whereas this parasite was found originally in the adipose tissue of the hog, Prof. Fletcher has found it in all the organs of the body—a statement which is in a measure borne out by the researches of Mr. Morris, who had found that it was not confined to the fat alone. In a third communication, Prof. Fletcher stated he had discovered the parasite in the urine of the animals he had examined. It was therefore very reasonable to suppose that this worm had given rise to the formidable disease referred to. In answering the question how it was brought about, we might be guided by our knowledge of the structure and development of *Trichina;* and it was not difficult to perceive that *Stephanurus* must set up an enormous amount of irritation during its migration through the tissues of the animal in which it had taken up its residence. But whereas, in the case of *Trichina*, the parasite migrated from *without*, and passed from the intestinal canal through the connective tissues towards the muscles, thus causing death by the small wound produced, *Stephanurus* did not, as far as is at present known, thus migrate from without, but from within; for, as Mr. Morris states, he has found individual parasites singly, and not encysted, in all parts of the fat. The most complete condition of the animal, however, was the encysted form. Dr. Cobbold then described the manner in which the female parasite probably formed a cyst, depositing the eggs in its interior; the diseased symptoms being produced by the migration of the progeny. He proceeded to say that as swine will eat swine, it followed that a cyst existing in one pig could be swallowed by another pig, and in that way the cyst could be taken into the intestinal canal, and be the means of propagating disease. This singular parasite, therefore, which had only once before been described, and which was mentioned only in an old book, now turned out to be one which would probably excite as much attention as *Trichina*. Further, no one could say that this parasite may not eventually find its way into our own bodies, for if we eat underdone American pork we should be liable to swallow the embryos, and they are just as likely to be developed in the human body as some other parasites found in the pigs. There was, however, this protection in the case of *Stephanurus*, that the most ordinary care in inspecting the meat brought to our tables would enable us to avoid swallowing any portion which contained a cyst. Dr. Cobbold then expressed his thanks for the opportunity

which had been afforded to him of examining this parasite, the genetic relations of which, though obscure, were full of interest to those who pursued this department of science.

The President, alluding to Mr. Hope's memoir "On Sewage Irrigation," asked Dr. Cobbold what were his views on the subject, specially in regard to the paper.

Dr. Cobbold replied, briefly stating that the British Association Committee having invited him to inspect an animal fed on Mr. Hope's farm on sewage-grown grass, he had carefully examined the animal, and had not found a single parasite of any description in it; whereas in the ox, under ordinary circumstances, one would expect to come across one or more of the score of different species liable to infest that ruminant. Here, however, there were none. But although this fact was, as far as Mr. Hope was concerned, an agreeable one, it did not, he thought, alter his (Dr. C.'s) original position in regard to the question, because it so happened that Mr. Hope's farm was a perfect model of what a sewage farm ought to be; and because the soil, being very porous, was eminently favourable for taking in any germs that might happen to be in the sewage. The case, however, was exactly the reverse on the Croydon farms, where there was a pestilential swamp, on which the grass was covered with deposits, and where eggs must inevitably be present if such eggs existed in the sewage. And though some people thought that because those eggs had not been found in the sewage after it had been distributed, no eggs were there; he asserted that millions of ova of *entozoa* were passed by persons in this metropolis, and it followed that numbers of those entozoa must find their way into the sewers, and be thence conveyed to the fields. To the question, Are ova carried likewise? the only answer that could be given was, that the common mode of the propagation of germs of *entozoa* must be through those channels, for they could go through no others; and under ordinary circumstances the eggs must go into the animal feeding upon the sewage-grown grass. His original statement therefore as to the mode in which the germs were spread abroad was not overthrown. The animal exhibited by Mr. Hope had been so carefully tended and reared, that it was next to impossible that any ovum could be brought into contact with it; and indeed the assiduous care bestowed upon it had served to deprive the animal of the privilege of harbouring even those parasites which would otherwise have been found in the body.

The President read a paper "On the *Form* and *Use* of Facial Arches in the Salmon."

Mr. H. Lee asked the President at what stage or period of the development of the embryo salmon the "blow-hole-like" organ which it was known did not exist in the adult fish was obliterated and ceased to be perceptible.

The President said, in the specimens of embryo salmon which he had examined he had been able to get at a very early stage in which he could discern a flat white band going round the yelk, and one eye-speck was visible. Before hatching he got two more stages, and in the second stage the ear-opening or spiracle became obliterated. There was a great difference between the salmon and the frog, for

he got his fourth stage in the development of the salmon when the first stage of the frog was observable. In the salmon, a few days after the yelk mass had undergone segmentation, the form of eye-ball and the involution of the eye-ball was very rapid. So also the after changes were intensely rapid. He had seen a little head, about the size of a pin's head, which began to emerge as the egg burst, and then the brain vesicles took their proper form. He would say one word in reference to the salmon. It was much nearer to the reptile or bird than the frog. The frog was very gradually developed, was of a very low type, and yet was so ambitious that it underwent metamorphoses near the parts of the face which brought it near to the mammal. The salmon was about half-way between the ganoid fishes and the perch. It seemed to form a much better connecting link between birds and reptiles than the frog did.

Mr. Stewart said he understood the President to say that he first used spirits to harden the preparations, and then put them into chromic acid. He (Mr. S.) had made a few experiments on reagents lately. He put a piece of brain into chromic acid, and in the course of a few weeks it had become completely vacuolated and hardened into a spongy-looking mass. It struck him that it would be well to recognize the possibility of such changes occurring in the transfer from alcohol to chromic acid.

The President said, care must be taken not to keep the preparations too long in the chromic acid, as under such circumstances they became very friable.

Mr. J. Beck wished to say a word or two in regard to a subject which had come before the Fellows in the Journal of the Society. There had been a review of a work purporting to be the 'Transactions of the Microscopical Society of Chicago.'* He had promised his friends there to take the earliest opportunity of informing the Society that they had not the slightest connection with the publication in question. It was a mere printer's speculation. He took the opportunity also of bearing testimony to the deep interest shown in microscopical research in America. The Americans displayed on this subject the same energy which characterized them in all matters in which they engaged. They made some very good instruments, and a very large number of persons were interested in their practical use. In a great many of the large cities of the States microscopical societies flourished, and conducted their affairs in a most admirable

* We regret that, owing to our absence, Mr. Beck's remarks were left uncontradicted. His statements contain a series of errors, and so far as any accuracy pertains to them they are a mere repetition of what has been already in print in the Journal. In the first place, there has not been any statement made to the effect that the journal reviewed—which was most severely reviewed—had any connection with the Chicago Society. In the next place, in our number for May last we published a letter from the Corresponding Secretary of the State Microscopical Society of Illinois (not of Chicago, as Mr. Beck has incorrectly expressed it) thanking us for his knowledge of the journal in question, which he was ignorant of till our notice appeared. Finally, in our September number we gave a full account of the nature of the 'Lens' which Mr. Beck alluded to at the October meeting. We regret being compelled to call attention to the matter, but Mr. Beck's charge leaves us no alternative.—Ed. 'M. M. J.'

manner. He adduced the conduct of the Government at Washington as an example for our own to follow, in taking note of any of their naval officers who displayed scientific tastes, and in furnishing them, when sent to foreign stations, with microscopes; the results of their researches being regularly transmitted to head-quarters, and, after being carefully examined by competent judges, were preserved, if considered worthy of preservation.

Mr. E. Richards exhibited a novel and simple form of "erecting mirror," made to surmount the eye-piece. It consists of a glass reflector, platinized on the front surface, thus getting rid of the second image, always seen when an ordinary silvered surface is employed. The little "erector" can be adapted for the small sum of five shillings to any instrument, and, it need scarcely be explained, will be of great service in making dissections or in viewing live objects, in a trough, as the microscope can be maintained in the upright position. The dissecting needles or knives are not reversed by it, as might have been expected; for after having adjusted the focus, by simply turning the mirror on its axis one quarter from the right hand, the needle held in the right hand is immediately brought into a proper position. The definition or perfection of the image is in no way impaired, and therefore it will be found useful in drawing as well as dissecting.

Donations to the Library and Cabinet, from June 7th to Oct. 4th, 1871:—

	From
Land and Water. Weekly	*The Editor.*
Journal of the Society of Arts. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*W. W. R.*
Journal of the Linnean Society, No. 3	*Society.*
Transactions of the Linnean Society, Vol. XXVII., Part 3	*Society.*
Observations and Experiments with the Microscope on the Chemical Effects of Chloral Hydrate, &c., on the Blood. By T. S. Ralph, M.R.C.S.E.	*Author.*
Transactions of the Natural History Society of Northumberland and Durham	*Society.*
Popular Science Review, No. 40	*Editor.*
Kalendar Companion to the Peerage and Baronetage, &c. 1870	*J. W. Stephenson, Esq.*
Clergy List for 1870	*Ditto.*
Proceedings of the Academy of Sciences of Philadelphia. 1870	*Academy.*
Smithsonian Report for 1869	*Institution.*
Transactions of the Connecticut Academy of Arts and Sciences, Vol. I., Part 2; and Vol. II., Part 1	*Academy.*
Zum Bane und der Natur der Diatomaceen. Von Dr. Adolf Weiss	*Author.*
Bulletins de l'Académie Royale des Sciences, &c., de Belgique. 1870	*Academy.*
Annuaire de l'Académie Royale des Sciences, &c., de Belgique. 1870	*Ditto.*
Sull' ultimo Stadio del Colera Asiatico. Del Professor F. Pacini	*Author.*
Le Globe. Tome X. 1871.	

Donations to the Library and Cabinet—*continued.*

	From
Berichte des Naturwissenschaftlich-medizinischen Vereines in Innsbruck. 1870 and '71.	
Journal of the Quekett Club	*The Club.*
Quarterly Journal of the Geological Society, No. 107	*Society.*
Report on Photographing the Soft Tissues by Sunlight, &c.	*Surgeon-General's Office, U.S.*
Intellectual Observer, 47 numbers	*M. C. Hardy, Esq.*
Quarterly Journal of Microscopical Science, 28 numbers	*Ditto.*
Monthly Microscopical Journal, 24 numbers	*Ditto.*
Science Gossip, 67 numbers	*Ditto.*
Recreative Science, 10 numbers	*Ditto.*
Set of Ten Original Drawings of Insect Scales. By Dr. Maddox	*Author.*
Three Photographs on Glass showing the striæ on *Amphipleura pellucida* taken with a $\frac{1}{8}$ objective of Tolles. By Col. J. J. Woodward, U.S.A.	*Ditto.*
Fourteen Photographs of the Podura Scale. By Dr. Maddox	*Ditto.*
The Cruise of the 'Norna.' By Marshall Hall	*Ditto.*
Journal of the London Institution, No. 7	*Institution.*
Mud from Narakol, near Cochin, West Coast of India	*Major-Gen. Worster.*
On the Structure and Development of the Skull of the Common Frog. By Wm. Kitchen Parker, F.R.S.	*Author.*
Logan's Simple Microscope, with three Powers	*James N. Logan, Esq., U.S.A.*
Twenty-four Slides of Intestinal Worms, &c.	*The Agricultural Society of New South Wales.*
Four Slides of Fossil Sponge spicules	*W. Vicary, Esq.*
Four Slides of Diatoms from the Valencia Deposit. Mounted for the Society by	*C. J. Fox, Esq.*

R. L. Maddox, Esq., M.D., was elected an Hon. Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

August 10th.—Ordinary Meeting. Mr. Hennah, Vice-President, in the chair.

Messrs. Boxall, A. H. Cox, W. H. Hallett, Hamblin, Walsh, and Dr. Knightley were elected ordinary members.

Mr. Wonfor read a paper entitled "Is *Bombyx callunæ* a distinct species, or only a variety of *Bombyx quercus?*"

After describing the differences between moths and butterflies, pointing out the peculiarities of the Bombycidæ, and minutely describing the life history of the two insects, *B. callunæ* and *B. quercus*, Mr. Wonfor classified the distinctions drawn by entomologists between the two: as of size, the first named being considered larger; of time in coming to maturity, the one taking two years, the other only one; of the difference in the plants; of the difference in the markings of the young larvæ, the one having orange triangles, the other orange and white lozenges; and of the different coloration and markings of the perfect insects. On the point of size, he had found an average of

either showed great diversity in size, colour, and markings. Difference of time was of little account, for undoubted southern insects had taken two years to complete their life history, and all entomologists knew that insects of the same brood, in this and other families, would stay one, two, three, and even five years in chrysalis. Even as regarded the difference in markings among the young larvæ, the greatest variety was noticed in larvæ taken from the same hedge-row; in fact all the points of difference pointed to a *climatic* variety; even in the case of colour, the tendency with insects, like other animals, was to acquire darker and duller hues as they advanced north, and lighter and brighter as they went south. A stronger point than all was the fact that he had succeeded in drawing up southern males with a northern female.

Taking advantage of the wonderful power possessed by the females of some groups of attracting the males from long distances and in great numbers, he had, by retarding, in a cold room, the time of emergence, got a female out on the 20th of July. This, taken to Hassock's Gate the same afternoon, about 4 o'clock, when there was but little sun and wind, had attracted males of the southern insect. On the principle that, among the insect tribe, the males of the same species only were attracted by the female, he considered this went far to prove the point that *B. callunæ* was only a *climatic* variety of *B. quercus*, and not a distinct species. Though occasionally hybrids were found in nature, the rule was for members of the *same species only* to pair. Had he been able to keep a female back a fortnight later, he doubted not he might have brought up nearly a hundred males of *B. quercus*, drawn by a sense either of smell of a very acute character, the organ of which had not yet been satisfactorily pointed out, or by some other sense, not yet localized or named by the naturalist.

The point had not been cleared up earlier simply because, relying on some of the points of difference before mentioned, no one, as far as he knew, had kept back northern females, and tried the experiment of seeing whether they would attract southern males.

August 24th.—Microscopical Meeting. Mr. M. Penley, Vice-President, in the chair; subject "Polyzoa."

Mr. R. Glaisyer announced the receipt for the Society's cabinet of 12 slides from Dr. Hallifax, 12 from Mr. T. Curties, of Holborn, 6 from Mr. Sewell, 6 from Mr. C. P. Smith, and 9 from Mr. Wonfor.

Votes of thanks were passed to the donors.

Dr. Hallifax, introducing the subject for the evening, said the Polyzoa were a proof of what might be done by patient investigation, for though at one time supposed to occupy an obscure position in the animal series, being classed among the polyps, by the comparatively recent researches of the last forty years they had been raised into a higher class of the animal kingdom. The greater number of them being contained in a horny polypidom, this, and not the animals contained therein, was chiefly studied, and the animal, or more important part, overlooked. By close observation and attentive study during the last forty years, Thompson, Farre, Milne Edwards, Grant, and

others, had removed them from the radiata in which they were placed by Cuvier, and elevated them to the mollusca, simply from the fact of their being proved to possess a higher organization. For some time they were known to possess a mouth and retractile tentacles, but no second or anal aperture could be detected, hence it was thought the one opening served both purposes. It had clearly been made out that the alimentary canal, or stomach, folded on itself, and was terminated by a second orifice close by, but distinct from the mouth, thus proving they were of a higher organization than the lower zoophytes, which had but one orifice. In the Bowerbankia, which possessed a very transparent envelope, a muscular structure, nervous ganglia, a representative of the liver, and a circulation of the nutritive fluid through the whole body had been made out, thus bringing them in connection with the mollusca; the more interesting, because, in external character and mode of growth, they were considered identical with the zoophytes; but microscopical examination had proved that the sea-mats of our shore were closely allied to the oyster and mussel. The tentacles, too, in which circulation could be detected, were ciliated—a state of things not found among the polyps.

The mode of reproduction was threefold: by germination, *i. e.* by buds; by ova; and by fissure or division; the last the most rare, the first the most common method.

To the microscopist they were exceedingly interesting, for while the skeleton exhibited great variety of form and beauty, scientifically they possessed a higher value, as showing what patient and enduring observation and skill might accomplish.

The Black Rock was a good hunting ground, but the masses of sea-weed washed up from deep water after stormy weather would supply many forms.

Mr. Wonfor mentioned that two methods were adopted for procuring and preserving specimens with their tentacles expanded; one was by plunging the specimen in cold fresh water, which killed and often caused them to exsert their tentacles, the other was to watch for the protruding of the tentacles in salt water, and to add spirits of wine drop by drop; this had the effect of killing the creatures with their tentacles expanded.

The meeting afterwards became a conversazione, at which some very beautiful preparations of Polyzoa and Anthozoa were exhibited by Dr. Hallifax, Messrs. Sewell, R. Glaisyer, and Wonfor.

Mr. Wonfor also exhibited specimens of Polyzoa and Anthozoa mounted on paper to show the form of the Polypidom.

September 14th.—Annual Meeting. Mr. F. Merrifield, President, in the chair.

The following gentlemen were elected officers for the ensuing year:—President, Mr. W. M. Hollis, J.P., M.R.C.S.; Committee, Dr. Badcock, Messrs. Haselwood, Sawyer, C. P. Smith, G. Scott, and R. Glaisyer; Treasurer, Mr. T. H. Horne; Hon. Secretaries, Messrs. T. W. Wonfor and J. C. Onions; Hon. Librarian, Mr. Gwatkin. The name of Mr. Merrifield was added to the list of Vice-Presidents.

From the Eighteenth Report of the Committee it appeared the

affairs of the Society were in a prosperous condition, there being a balance in the hands of the treasurer of 12*l.* 0*s.* 1*d.*, after spending 19*l.* 5*s.* 11*d.* in the purchase of new books and periodicals, the committee retaining a balance to defray the expenses of printing a new catalogue. Considerable additions had been made to the library, which now numbered over 700 volumes, by purchase, and by donations from Drs. Addison, Stevens, Wallich, Messrs. T. Davidson, Hennah, Roper, and the Secretaries of the Belfast, Eastbourne, Lewes, Maidstone and Mid Kent, and Quekett Societies. Additions had been made to the microscopical cabinet of 129 slides from Messrs. Curties, Eden, Gwatkin, Marshall Hall, Hennah, Neate, C. P. Smith, Wonfor, and Dr. Hallifax. The Monthly Microscopical Meetings had added greatly to the value of the Society. The committee recommended the holding a conversazione during the year. The thanks of the Society were due to the Brighton and Hove Dispensary and to the Medico-Chirurgical Society for the use of their room, and to those gentlemen who had exhibited microscopes and specimens, read papers, and contributed to the library, album, and cabinet. There had been seven field excursions, one by special invitation from Mr. Grantham. The annual excursion was to Arundel, where the Mayor, W. W. Mitchell, Esq., hospitably entertained the Society at luncheon, and His Grace the Duke of Norfolk granted permission to see the private gardens and grounds attached to Arundel Castle. An abstract of the Scientific Proceedings showed that papers had been read by Dr. Badcock on the "Gulf Stream," by Dr. Addison on "*Daphnia pulex*," Dr. Dawson on "Sphores," Dr. Hallifax on "Bone" and "Polyzoa," Mr. Ackland on "A Neutral Tint Selenite Stage," Mr. Hennah on "Illumination" and "Gundlach's Objectives," Mr. Howell on "Excavations through the Post Pliocene at Brighton" and the "Brighton Cliff Formation," Mr. Merrifield on "Tree Planting in Brighton—suggested improvements," Mr. G. Scott on "Rude Flint Implements," Mr. Sewell on the "Use of the Polariscope in the Determination of Structure," Mr. C. P. Smith on "Lichens," and Mr. Wonfor on "Shell Structure," "What is Coal?" "The Annual Excursion," and "Is *Bombyx callunæ* a Species or a Variety?" There had been three evenings for exhibition of specimens, when many interesting objects had been exhibited, and at the Microscopical Meetings practical instruction had been given to the members in "Mounting," "Section Cutting and Making," "Illumination," &c., by Dr. Hallifax, and Messrs. Hennah and Wonfor. Votes of thanks were passed to the Secretaries, Librarian, President, and outgoing officers.

The meeting then became ordinary, when the new President, Mr. W. M. Hollis, took the chair, and Messrs. D. Friend, Dick, and W. S. Smith were elected members.

A number of very interesting specimens were then exhibited by Messrs. Penley, Saunders, Hennah, Howell, Ardley, Sewell, Nourse, Goss, and Wonfor; among the most striking were a specimen of the new burnet moth *Zygæna exulans*, exhibited by Mr. Goss, and of the rare moth *Deiopeia pulchella*, crimson speckled, taken by Mr. Wonfor in a stubble field at Hove, on September 4th.

September 28th.—Microscopical Meeting. The President, Mr. W. M. Hollis, in the chair. Subject "Diatoms."

Mr. Wonfor, who introduced the subject for the evening, said he approached it with some diffidence in the presence of some, especially of Mr. Hennah, who had devoted much time to the study of diatoms, and who knew much more about them than he did.

Diatoms were unicellular algæ of a peculiar character, distinguished from other unicellular plants, and especially the desmid, to which they bore a great resemblance, by the possession of a silicious covering which, while it rendered them exceedingly brittle—hence their name brittle worts—also made them all but indestructible under ordinary circumstances. One great peculiarity with them was the fact that if the internal cell membrane became exposed to water, it secreted a silicious covering, and if the plates forming the frustule became separated a plate of silex began to form, and became what was termed the connecting membrane.

The frustules were either free, *i. e.* moved freely in the element in which they were found, from which circumstance they had been called animals; adherent or attached to the substances on which they grew, or aggregated; in this last case they cohered either by their angles, were provided with a gelatinous pedicel, which united the frustules together; or they were enclosed in great numbers in a general thallus.

The separate frustules as seen from a front or side view presented very different appearances; in fact, some like the *coscinodisci* were circular in a front and nearly rectangular in a side view, with a line down the centre, which showed the junction of the two frustules. The cell contents of the frustule were a viscid protoplasm called the *endochrome*, generally of a brown or yellowish colour, containing globular and granular bodies, of which the latter had been seen in some cases to rotate within the cell, so forming a species of cell-circulation.

The mode of increase was by self-division and by conjugation; in the former, the valves separated, the cell contents aggregated on opposite sides of the frustules, the primordial utricle folded in, became contracted, and eventually separated, at the same time a new silicious valve was secreted by each half, and the result was two diatoms in the place of one. This mode of growth was very rapid. In multiplication by conjugation, two frustules lying near each other opened at their sutures and exuded their cell contents, which coalesced, while the whole was involved in a gelatinous substance, from which sprung a frustule of larger size than the parents, to which the name sporangial frustule was given.

The silicious valves had, from their markings, always been favourites with microscopists, and though some complained of too much time being devoted to diatoms, yet the question of the nature of their markings had been the means of improving objectives, and had also led to the designing various forms of illuminating apparatus more or less simple or complicated.

A discussion ensued, in which the President, Messrs. Horne, Robertson, Wonfor, Glaisyer, and Hennah took part, the last-named gentleman drawing especial attention to the researches of Dr. Maddox,

which, when published, would throw great light on the life history of diatoms.

The evening then became a conversazione, when living diatoms and a great variety of silicious valves from different localities were exhibited by Dr. Hallifax, Messrs. Hennah, R. Glaisyer, and Wonfor.

Mr. Hennah also exhibited some exquisite micro-photographs of diatoms taken by Dr. Maddox, some of them magnified 3000 diameters.

South London Microscopical and Natural History Club.

An Ordinary Meeting of this Club was held at Glo'ster Hall, Glo'ster Place, Brixton Road, on Tuesday, August 15th, at half-past seven o'clock in the evening. Dr. Braithwaite, F.L.S., presided.

Four new members were balloted for, and duly elected.

Dr. Hector Helsham, F.R.C.S., read a paper "On the Employment of the Microscope in Analysis," of which the following is an abstract:—

The subject of analysis embraces a vast field, for the microscope is pre-eminently the instrument of analysis, and every application of it an act of analysis, whether working out the minutiæ of structure, developing the growth of the smallest organism, or defining the constitution of some inorganic particle. It will be advantageous therefore to limit the application of the term to the investigation into the qualities of things, leading up to the establishment of facts, in contradistinction to the tracing down to the originals of form and structure in physiological and biological sciences.

It is my purpose, then, to consider the application of the microscope to the detection of the minutest organisms in nature, its application to the detection of the results of diseased action, to adulteration, and the detection of crime.

To our unaided vision, animals and plants present differences of form and structure by which we are able to distinguish one from the other, and by closer examination we find deeper evidences of variety in their organization, and when we extend our survey still deeper, the more extraordinary is the amount of minute organization in every animal or vegetable production. These differences rightly observed and rightly interpreted by the microscopist furnish an unfailing identity to every portion, however minute, of vegetable and animal tissues, and enable us to refer them to their origin. Observe how much has grown out of the study of the starch-granule, how every structure that contains it may be distinguished; contrast, for the sake of distinction, the starch-granule of the potato with that of rice, and note the ready point of difference here established—applicable easily to the examination of the purity of wheaten flour; contrast also the granules of Maranta, or West Indian arrowroot, and the granules of the turmeric-root, favourite articles employed in adulterating spices. We have thus by their study a reliable typical character on which to verify our judgments. The starch-granule will also illustrate the extent of study which may concentrate around one special object as to

its constitution and structure, and the many opinions that may originate from the observations upon it; whether the hilum, or central point seen on its surface is due to the adhesion of the starch-granule to the cell-wall which contains it; whether it is a cavity, or a nucleus, or simply resulting from a phenomenon of the refraction of light; whether the concentric markings are due to layers superimposed on the first-formed material, or deposited within each other as newly formed, or are merely due to the plaitings or foldings-in of a vesicular membrane; whether the granule is contained in an envelope of denser material, or whether it is of a homogeneous structure throughout. It illustrates also the effects of various modes of illumination as seen by transmitted, reflected, or polarized light.

The origin of life is now a very prominent study, and embraces very many hypotheses which depend very much on microscopic observation for their tenure; the question of the mode of origin of living matter being inextricably mixed up with another problem as to the cause of fermentation and putrefaction. Baron Liebig holds that fermentation is purely a chemical process, but the doctrine of M. Pasteur maintains that fermentation can only be initiated by the agency of living things—*omne vivum ex vivo*—the theory of spontaneous generation. Again, there is the theory of heterogenesis, which imagines that when the vital activity of any organism is on the wane, its constituent particles, being still portions of living matter, are capable of individualizing themselves, and growing into the low organisms in question; and, again, a new theory by Professor Bastian, that new life may burst forth, *de novo*, in certain fluids containing organic matter. These hypotheses, however, really range themselves under the vital and the material doctrines, and it is for the microscope to solve the question.

Of all subjects of immediate and vital interest to the community at large, in which the microscope must necessarily again be the principal and chief arbitrator, there is none so important as the consideration of contagious diseases. With the Asiatic cholera at our gates, it is indeed well that we should know what we have to contend with, for the knowledge of a disease is its cure. At present the germ theory of disease is the prevailing one, and disinfectant treatment, and disinfecting precautions, are the chief reliances of the day; but we fail to find from late experiences that the totality of zymotic diseases is diminishing, and there are many who have yet held back from the acceptance of this theory, denying the proofs as not yet demonstrated; and here we have again the never-failing aid of the microscope in its analytical power to judge of what we can apply to its investigation. If we can only prove the existence of these germs as a *modus propagandi*, we have not only a ready means of explaining the spread of these diseases, but should also be armed with the means of destroying them, and the prevention of the scourging epidemics with which the world has ever been afflicted. The vital importance of these inquiries may be realized by the known results, for we learn from statistics that the deaths from zymotic diseases in England and Wales amount to upwards of 111,000 per annum, out of a population of 22,000,000, the total deaths being

under 500,000; and the want of knowledge, even among very intelligent persons, concerning the practical requirements for limiting the spread of contagious diseases is deplorable, so that in epidemics the scourge is sometimes fostered and spread by the very persons in charge of the sick, sometimes by the patients themselves being allowed to mix with the healthy and distribute far and wide the germs of disease. Heads of families are not always aware that a child who has completely recovered from scarlet fever, and is in fact well, may communicate it to half the children he comes in contact with, unless he be placed in quarantine for three months at least, by which time there is reason to believe that all active contagious particles will have become obliterated.

This germ theory of disease has lately been brought into considerable notice in consequence of Professor Tyndall's lectures at the Royal Institution on dust and haze, in which he sought to prove that the particles floating therein are germs of animal and vegetable, probably the seeds of infectious disease. Professor Beale, however, has, by the aid of a $\frac{1}{50}$ inch objective, discarded the vegetable germ theory, and propounded a bioplasm or minute morsel of germinal matter, possessing a separate vitality, distinct from the organism into which it may become absorbed, and developing within the fluids of the containing organism.

Passing on now from these subjects to one of more domestic interest, the detection of adulteration in our foods, drinks, and drugs, opens up to us all a ready and useful application of the microscope. Until some legislative act shall provide public analysts and inspectors, so long will adulteration greatly prevail. It is exceedingly difficult, if not impossible, to procure wholesome, unadulterated food: and although an Act of Parliament has existed since 1860, not a single conviction has been obtained under it in the metropolis.

The adulteration of tea gives us a very good example of the extent to which what is called sophistication is carried on. Tea is doubly dealt with in the way of fraud, for it receives a first instalment at the hands of the producers, and a second at the hands of the dispensers at home; although the latter may receive the credit of doing nothing very venomous, but chiefly by way of diluting a good with an inferior article, and colouring up a common caper into fine gunpowder by refacing the tea with chalk and prussian blue. The adulterations practised by the exporters in China are in the glazing, the use of many deleterious mineral substances, such as plumbago (carburet of iron), chrome yellow (chromate of lead), Scheele's green (arsenite of copper), in drying up the leaves of other plants with the tea, such as those of the willow, the beech, the elm, the plane, the sloe, &c., and in redrying the leaves of tea that have already passed through the pot. But the most startling, because the newest, is what is called the Maloo mixture, from its resemblance to the tan with which the Maloo race-course is strewed. It is extensively employed for mixing with tea, or even sold as tea itself; and it is said that 30,000*l.* worth of this wretched compound was recently on its way to this country. The Chinese collect all the used-up tea-leaves they can get, and keep them in heaps

near their residence; these heaps are at the right time spread out to dry and shrivel; and, after facing, are leaded, packed, and sold for dispatch to this country, often mixed with iron-filings, sand, &c., to increase the weight of the chest. As with tea, so with coffee; chicory is made up in the form of the coffee-berry, so as to deceive even the diligent housewife who grinds her own; and in ground coffee you get bad flour, roasted beans and peas, acorns, and even mahogany saw-dust; and the chicory itself is diluted (the mild term) with black-jack or roasted old sea biscuits, and croats, the spent tan of the tan-yard. Our bread is such dry innutritious stuff as inferior starches and alum can make it; our milk a sky-blue fiction, with cream that falls to the bottom, a thief in the nursery, and a robber of the invalid. Our microscopes may be our own inspectors, and surely something of a wholesome check to tradesmen would arise from the constant practice of analyzation of food, as the notice of any particular specimen of adulteration would act as a caution to check the tide of imposition from which we all so severely suffer, both in health and pocket.

Turning now to the detection of crime by the aid of the microscope. In cases of poisoning, the crystals may be obtained from the victim, and shown to the jury; in poisoning by arsenic, by strychnine, by opium, by corrosive sublimate, oxalic acid, and other mineral poisons, this may be, by care, easily effected. And in many other cases of judicial inquiry the microscope brought to the analysis may positively decide the verdict. In a notorious case that has lately been before the public, the presence of blood-stains was proved on the dress of the accused, and here the utility of the spectroscope was manifestly shown—a scientific adjunct to the microscope, which is yet only in the infancy of its application, but yields such well-marked and characteristic spectra, that there are few subjects to which the spectrum microscope can be more advantageously applied than the detection of blood-stains. A millionth of a grain will show the characteristic absorption-bands.

Thus we have passed in review the generalities of the subject of microscopic analysis, and it is my hope that sufficient has been shown to impress on the minds of my hearers how deeply important it is, to those who are engaged in any such pursuit, or to those who have not yet commenced some systematic investigation, to at once determine not to follow in paths already well trodden, but to open up new ones; not to seek to use our microscopes here by repeating experiments, but rather let us all vie in seeking to show the results of new and original work.

A vote of thanks was unanimously accorded to Dr. Helsham for his interesting paper.

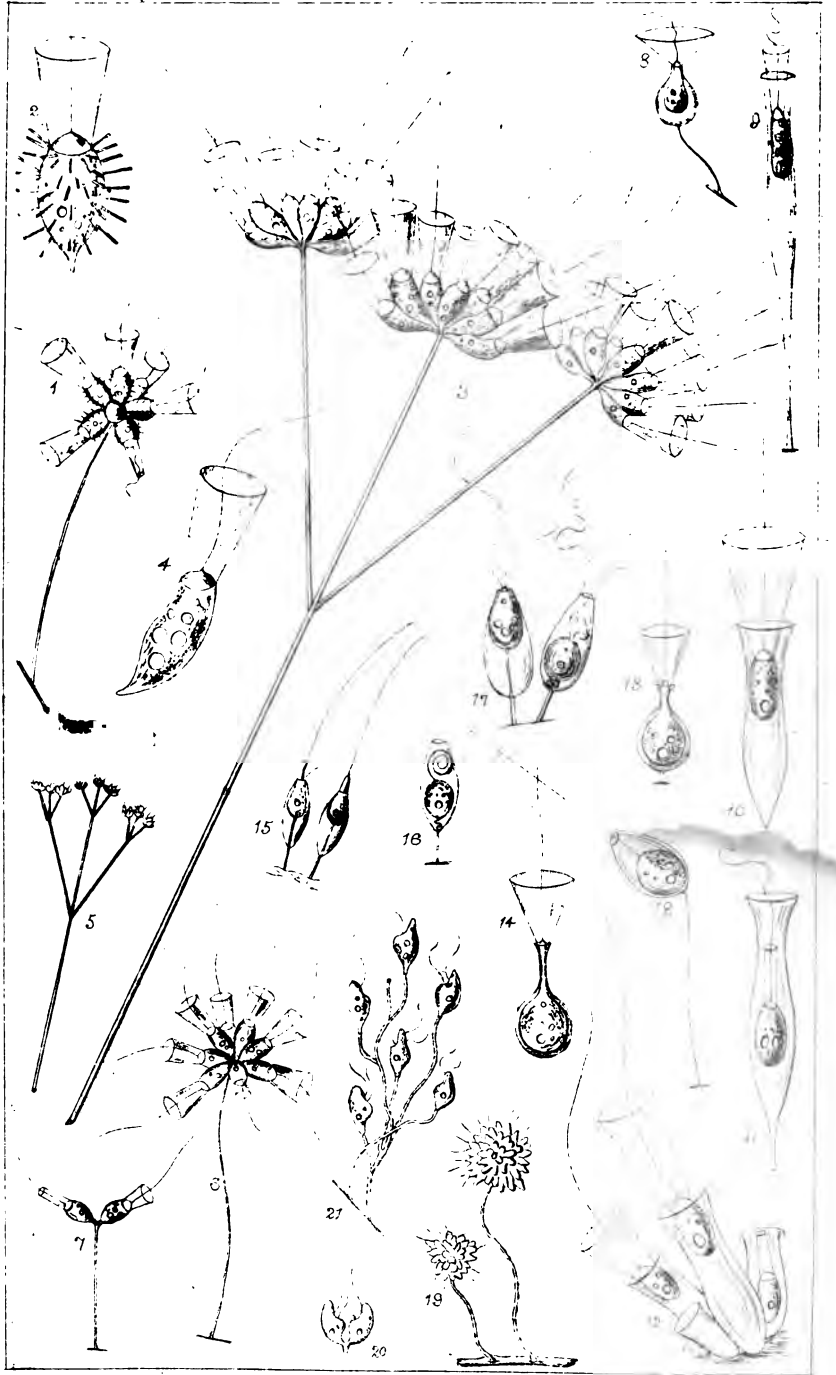
Excursions were announced on August 26th to Rainham, and on September 9th to the Victoria Docks.

The meeting then resolved itself into a conversazione, a paper being announced for the next meeting, on Tuesday, September 19th, at half-past seven o'clock in the evening, by Charles Stewart, Esq., M.R.C.S., "On some of the Lower Forms of Animal Life."

Hull Scientific Association.*

The inaugural address was delivered before the above association on the 13th instant by the President—subject: The Work of the Microscope. The speaker pointed out that the proper work of a local society was local, that the flora and fauna of the locality should be its first care, and hoped that the members of the new association would carefully examine and, as far as possible, tabulate the microscopic organisms of the neighbourhood. A list of the microscopic fauna and flora of each district occupied by a Field Club or Natural History Society would, he thought, be of great service to professional botanists or zoologists engaged in the study of the distribution of animals or plants in time and space; but he hoped that members would carefully remember that cataloguing was not very intellectual work, and that no member would be content unless he learned something of the life history of the organisms whose names and external morphology he studied. Passing from this branch of his subject, the speaker glanced briefly at the departments of work in which certain members of the association were engaged, and pointed out how interdependent all branches of science were now seen to be. The member who studied crystallography with the aid of the polariscope would soon discover that the botanist and zoologist made demands upon his knowledge, because they found when they used the polariscope in the investigation of organic structures, the same phenomena of interference and refraction with which the crystallographer was so familiar. The greater part of the address was occupied by a dissertation on the use of the microscope in unravelling the mystery of plant life, or rather of the life history of plants. The speaker began with a *Torula cerevisse*, and having described its mode of multiplication, passed on to the development of some of the lower algæ, such as Palmoglœa, and thence to the development of cells in the young leaves of Anacharis, and concluded by pointing out some direction in which microscopic botany might do great service. At the conclusion of the address, the meeting resolved itself into a conversazione.

* This is a new association, and is formed for the purpose of affording mutual aid and instruction in science. The constitution of the Society is open, but at present nearly all the members are devoted to microscopy. The report is furnished by Mr. C. P. Gibson, M.P.S., Hon. Sec.



W.S. Kent del.

W. West & Co. imp.

New Infusoria.

THE

MONTHLY MICROSCOPICAL JOURNAL.

DECEMBER 1, 1871.

I.—*Notes on Prof. James Clark's Flagellate Infusoria, with description of New Species.* By W. Saville Kent, F.Z.S., F.R.M.S., British Museum.

(*Read before the* Royal Microscopical Society, *Nov.* 1, 1871.)

The following is a brief synopsis of certain genera and species of Flagellate Infusoria described by Professor James Clark, of Pennsylvania, U.S., in the 'Memoirs of the Boston Society of Natural History,' vol. i., 1866,* with the diagnostic characters of new species interpolated and those of other previously-described ones

EXPLANATION OF PLATE CV.

Fig. 1.—A colony of *Codosiga echinata*, magnified 500 diameters.
„ 2.—A single zooid, detached, magnified 1500 diameters.
„ 3.—A colony of *Codosiga umbellata*, magnified 400 diameters.
„ 4.—A detached zooid, magnified 800 diameters.
„ 5.—Another colony of the same species, exhibiting a bi-tripartite ramification of the supporting pedicle, magnified 75 diameters.
„ 6.—A colony of *Codosiga pulcherrima*, magnified 500 diameters.
„ 7.—Another colony of the same species consisting of only two individuals.
„ 8.—*Salpingœca marina* (after Jas. Clk.).
„ 9.—*Salpingœca gracilis* (after Jas. Clk.).
„ 10, 11.—Two solitary and shorter individuals of the same species as observed by the author, magnified 1200 diameters.
„ 12.—A colony of still shorter individuals, in which the elongate, hollow peduncle is entirely suppressed. Magnified 1200 diameters.
„ 13.—A variety of *Salpingœca amphoridium*, attached by a short pedicle, and in this character approaching the stalked species *S. marina*. Magnified 1200 diameters.
„ 14.—An ordinary sessile individual of the same species, magnified 1200 diameters.
„ 15.—Two zooids of *Bicosœca lacustris* in the condition of full extension, magnified 1200 diameters.
„ 16.—A single individual of the same species, further enlarged, with the zooid retracted within its lorica, and exhibiting the spiral manner in which the flagellum is then disposed.
„ 17.—Two zooids of *Bicosœca socialis*, magnified 800 diameters.
„ 18.—*Bicosœca inclinata*, magnified 600 diameters.
„ 19.—Two compound colonies of *Anthophysa solitaria*, magnified 400 diameters.
„ 20.—Two zooids of the same species, derived from the longitudinal fission of a single individual, and still connected with one another at their base. Magnified 1500 diameters.
„ 21.—A compound colony of *Anthophysa laxa*, magnified 900 diameters.

* Reprinted in the 'Annals and Magazine of Natural History,' 4th ser., vol. i., 1868.

amended in accordance with the author's personal experience. Without exception the material furnishing these descriptions was obtained from a pond of moderate dimensions on the estate of Thomas Randle Bennett, Esq., of Wentworth House, Stoke Newington.

CODOSIGA, Jas. Clark (revised).

Bodies of animalcules ovate, one or a number seated at the termination of a fixed, slender, unretractile, simple or branching pedicle; the anterior and distal portion bearing a membranous, infundibular, retractile collar; the single attenuate, flexible flagellum originating from the centre of the area which it circumscribes. Contractile vesicles conspicuous, one or more in number. Increasing by longitudinal fission.

Codosiga pulcherrima, Jas. Clk. (revised), Pl. CV., Figs. 6, 7.

Animalcules from one to as many as eight or nine in number, attached to the primary pedicle through the medium of very short secondary ones; surface of the body smooth, length of the same, exclusive of the collar, $\frac{1}{100}$ mm. ($\frac{1}{2500}$ Eng. inch), breadth $\frac{1}{200}$ mm.; length of the primary pedicle, four or more times that of the body, of the secondary ones about half its diameter. Contractile vesicles conspicuous, two or more in number.

Hab. Fresh water, attached to *Myriophyllum* and *Conferva*. Pennsylvania, U.S., Jas. Clark. Stoke Newington, London, W. S. K.

Codosiga echinata, n. sp., Pl. CV., Figs. 1, 2.

Similar to *C. pulcherrima*, but the individual animalcules having the surface of their body beneath the collar beset with verticels of evenly-disposed stylate processes. Length of the body $\frac{1}{100}$ mm., breadth $\frac{1}{200}$ mm.

Hab. Fresh water, on *Myriophyllum* and *Conferva*. Stoke Newington, London, W. S. K.

Codosiga umbellata, n. sp., Pl. CV., Figs. 3, 4, 5.

Bodies of animalcules similar in structure to those of *C. pulcherrimus*, but of double the length ($\frac{1}{50}$ mm.) and more elongate outline, seated in groups at the terminations of a rigid tripartite, bi-tripartite, or occasionally quadri-partite, branching pedicle.

Hab. Fresh water, on *Myriophyllum* and *Conferva*. Stoke Newington, London, W. S. K.

SALPINGÆCA, Jas. Clk. (revised).

Animalcules inhabiting a transparent lorica or sheath, the anterior portion of the body provided with a membranous retractile

collar and bearing a single attenuate flexible flagellum. Lorica attached, sessile or pedunculate. Contractile vesicles conspicuous, one or more in number.

Salpingœca gracilis, Jas. Clk. (revised), Pl. CV., Figs. 9, 10, 11, 12.

Lorica cylindrical, solitary or in groups; expanding anteriorly, attenuate, in the form of an elongate hollow peduncle, or abruptly truncate posteriorly. Bodies of animalcules cylindrical, rounded at the two extremities. Average length of lorica $\frac{1}{400}$ to $\frac{1}{500}$ mm., breadth $\frac{1}{1500}$ mm., animalcules occupying one-third to one-half the length of its internal cavity.

Hab. Fresh water. Pennsylvania, U.S., Jas. Clk. Stoke Newington, London, on *Conferva*, W. S. K.

Salpingœca amphoridium, Jas. Clk. (revised), Pl. CV., Figs. 13, 14.

Lorica flask-shaped, having an inflated posterior portion and an attenuate narrow neck, sessile, or attached by a short pedicle. Body of irregular form, adapting itself to the outline of the lorica. Length of the lorica $\frac{1}{1300}$ mm., breadth of the expanded base $\frac{1}{2400}$ mm.

Hab. Fresh water, attached to *Conferva*. Pennsylvania, U.S., Jas. Clk. Stoke Newington, London, W. S. K.

BICOSŒCA, Jas. Clk. (revised).

Body enclosed within an ovate membranous lorica or sheath, to the bottom of which it is attached through the medium of a contractile ligament; no collar; one or two flagelliform appendages originating from the anterior extremity. Lorica usually pedunculate. Contractile vesicles one or more in number.

Bicosœca lacustris, Jas. Clk. (revised), Pl. CV., Figs. 15, 16.

Lorica elongate oval, narrowing at the anterior extremity, attached by a short pedicle. Body rounded posteriorly, rostrate anteriorly and bearing a single flagellum originating excentrically, curved and rigid in extension and a shorter stylate appendage. Length of lorica $\frac{1}{1300}$ mm., breadth $\frac{1}{2600}$ mm.; body occupying one-third to two-thirds of its internal cavity. Contractile vesicles two in number.

Hab. Fresh water, on *Conferva*. Pennsylvania, U.S., Jas. Clk. Stoke Newington, London, W. S. K.

Bicosœca socialis, n. sp., Pl. CV., Fig. 17.

Lorica elongate oval, half as long again as that of *B. lacustris*; pedicle not exceeding one-third or half its length. Body rounded

posteriorly, pointed anteriorly, not rostrate, and bearing two flexible attenuate vibratile flagella. Length of lorica $\frac{1}{60}$ mm., diameter $\frac{1}{120}$ mm.; body occupying about one-half of its internal cavity.

Hab. Fresh water, attached to *Conferva*. Stoke Newington, London, W. S. K.

Bicoxæca inclinata, n. sp., Pl. CV., Fig. 18.

Lorica ovate, set obliquely on a slender pedicle of twice its length. Body occupying two-thirds of the cavity of the lorica, flagellum single. Length of lorica $\frac{1}{60}$ mm., greatest diameter $\frac{1}{100}$ mm.

Hab. Fresh water, attached to *Conferva*. Stoke Newington, London, W. S. K.

ANTHOPHYSA, Duj. (revised).

Animalcules pyriform, obliquely truncate anteriorly and furnished with two flagelliform appendages, attached singly or in clusters to a simple or branching uncontractile stem. Increasing by longitudinal fission.

Anthophysa solitaria, Bory, Pl. CV., Figs. 19, 20.

Animalcules grouped in a cluster of forty or fifty at the extremity of a simple flexible thread-like stalk. Length of bodies $\frac{1}{200}$ mm.

Hab. Fresh water, on *Conferva*, &c. European Continent, Bory and Tresenius. Stoke Newington, London, W. S. K.

Anthophysa laxa, n. sp., Pl. CV., Fig. 21.

Animalcules disposed singly, excepting during fission, at the extremities of a loosely and irregularly branching flexible stalk. Length of bodies $\frac{1}{130}$ mm.

Hab. Fresh water, on *Conferva*. Stoke Newington, London, W. S. K.

Anthophysa Bennettii, n. sp.

Animalcules stationed singly or in pairs (during fission) at the extremities of a slender, rigid, and repeatedly dichotomously dividing stalk. Length of bodies $\frac{1}{200}$ mm., of the branching stalk 1 mm. and upwards.

Hab. Fresh water, attached to *Conferva*. Stoke Newington, London, W. S. K.

Monas termo, Ehr.

Having encountered the attached form referred to this species by Prof. James Clark, I differ from him in his assumption that

the animalcule possesses a distinct mouth, having on many occasions observed it to take in food on the lateral as well as the anterior surface of its body, it investing and engulphing its prey with an expansion of its protoplasm after the manner of Amœba. On numerous occasions I have also certified the presence of two instead of only a single flagelliform appendage.

ADDENDA.

In *Anthophysa* the interception of food is similar to that of *Monas*. In *Codosiga* and *Salpingœca* it takes place anywhere within the area circumscribed by the membranous collar, the discharge of fœcal matter being effected within the same limits.

II.—*Notes accompanying Three Photographs of Degeeria domestica, as seen with Mr. Wenham's Black-ground Illumination and a Power of* 1000 *diameters.* By Dr. J. J. Woodward, U. S. Army.

(*Read before the* Royal Microscopical Society, *Nov.* 1, 1871.)

Mr. Wenham having very kindly sent me one of the small truncated lenses designed by him to obtain, under certain conditions, black-ground illumination with high powers, I carefully tried it on the *Degeeria domestica* in the manner described in his paper in the July number of the 'Monthly Microscopical Journal,* using the immersion $\frac{1}{16}$th of Powell and Lealand as the objective. I had no difficulty whatever in obtaining the appearances of the scale described by Mr. Wenham and also several other aspects, and must regard the contrivance as a valuable addition to our means of studying semi-transparent objects with high powers.

The illumination of the scale by this method, when a coal-oil lamp was the source of light, was so brilliant that I thought probably it would be possible to photograph some of the more striking appearances. I obtained on the first trial three negatives, of which I send prints. The same scale is shown in each magnified 1000 diameters. The objective (the immersion $\frac{1}{16}$th) remained at the same cover correction, the position of the truncated lens and parabola was unaltered, and the different appearances exhibited resulted from trifling alterations in the position of the plane mirror by which the parallel solar pencil was thrown upon the parabola, and slight modifications of the fine adjustment. Of the prints sent, No. 2 agrees pretty well with Mr. Wenham's description. The same can hardly be said of No. 3, however, and Nos. 1 and 3 are only examples of some of the manifold results attainable by this method with which, indeed, almost as many appearances can be seen as with transmitted light.

The time of exposure required for these negatives was but three minutes. I infer from this and many other circumstances, that the semi-transparent scales are simply made luminous by the light passing into them from below, whence it results that the surface appearances are necessarily complicated by the optical properties of the structures beneath.

I understand this to be substantially Mr. Wenham's view also, and am therefore at some loss to comprehend the sense in which he speaks of the scales as being shown "opaquely" by this method.†

* P. 7.

† P. 7, and note *loc. cit.*

Note on the above. By F. H. Wenham.

In inadvertently making use of the term "shown opaquely," I did not wish it to be inferred that I considered this method as strictly an opaque illumination, which is understood when the light is thrown only on the upper surface of the object.

The truncated lens, or flat-topped parabola, first used by me in the way referred to above, gives such a brilliant luminosity to the object, on a jet-black field, that it has all the appearance of an opaque illumination, and perhaps on many objects the difference in apparent structure would not be material, and may be illustrated in this way: Suppose some semi-transparent body, such as a green grape, be let into a piece of black card; on holding this against a strong light, so that it enters sideways, the seeds and internal structure will be shown satisfactorily. If a side light is condensed down upon the object, the same internal structure will be seen, though not so perfectly on account of surface glare. When a side light is thrown into the body of an object either way, each dense particle that intercepts it serves to illuminate its neighbour, and so the rays are diffused in every possible direction, and if the structure contains particles actually impervious to light, they will not be seen like dark shadows as by direct light, but luminous, and in their natural colours. I consider this is the main principle—to send the light into the object in any *or all* directions beyond the angle at which rays from the source can enter the eye.

Dr. Woodward has kindly sent me the photographs referred to in the above note. No. 2, which quite agrees with my description, is in places very sharp and distinct, showing the intercostal striæ or bars plainly. Nos. 1 and 3 are somewhat blurred, and to my mind do not show structure satisfactorily. Knowing the difficulty of obtaining a photograph of an object of this character, merely from its own diffused light, I was much surprised at Dr. Woodward's remarkable skill in producing a perfect picture—a feat that I should have thought scarcely possible.

III.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

(*Read before the* Royal Microscopical Society, *Nov.* 1, 1871.)

Part II.

Before commencing the descriptive portion of our subject, it may be well to enter a little more into detail with respect to the histology of the interesting plants constituting the Sphagnaceæ. The published materials of which I have availed myself in the study, are the following, and to Prof. Lindberg, of Helsingfors, I am also deeply indebted for beautiful specimens of some of the rarer species.

1. Dozy—Bijdrage tot de Anatomie en Phytographie der Sphagna. 1854.
2. Schimper—Entwickelungs-geschichte der Torfmoose. 1858.
3. Lindberg—Torfmossornas byggnad udbredning och systematiska uppstallning. 1862.
4. Russow—Beiträge zur Kenntniss der Torfmoose. 1865.

The roots, which form only on the young plants, are chiefly of use in fixing them to floating objects, for as soon as branches shoot forth, a part of these forming each fascicle drop perpendicularly downward, and becoming appressed to the stem, are from their hygroscopic quality far more effective than true roots, in transmitting fluid to the other parts of the plants; while the dense masses formed by the aggregation of stems equally supersede the use of roots as fixing organs. As the stem increases in size, the simple flageller branches arise laterally from the uppermost leaves, and are crowded together into a head or capitulum, which supplies fascicles of branches to the stem below, by elongation of the internodes, and keeps up its stock of young branches by constant renewals from the growing point at the apex. The dichotomous ramification of a Sphagnum depends on the annual production of an innovation, which is a perfect repetition of the stem of the previous year, and derives its origin from one of the lateral branches of the capitulum, which rises upward and becomes elongated into a main axis.

The number of branches in a fascicle seems tolerably constant in each species, a part of these we may call the *divergent* branches, which proceed at a right angle from the stem, then bend about the middle and arch gracefully downward; the rest we will term the *pendent* branches, and these are longer, more attenuated, and fall down from their point of origin in the fascicle, and lie close to the stem. A part of the divergent branches become condensed and club-shaped to form the catkins of male flowers, and a few others become fruit branches.

The leaves of Bog mosses vary considerably on different parts of the plant: the stem leaves are distant from each other, and usually

reflexed against the stem, probably pushed back by the descent of the pendent branches; at their basal angles we also frequently observe appendages or auricles formed of larger perforated cells. The areolation of the stem leaves is wider than that of the branch leaves, and the prosenchym cells of the lower part are often altogether threadless, while one or more rows at the extreme base are small, hexagonal, vesicular, and coloured red or yellow.

The branch leaves are small, more densely reticulated, closely imbricated over each other, and very variable in form and size; this variability, however, is greatest in the pendent branches, where both they and their component cells become extremely elongated.

Russow, however, points out that the leaves on the *centre* of the divergent branches are very constant in form in the individual species, and that they all become narrower and more distant as they approach the apex of the branch. Moreover, the 3—5 lowest leaves at the base of the divergent branches are remarkably different in form from those which succeed them, and stand midway between them and the stem leaves; the typical form of these *intermediate* leaves is an irregular-sided, obtuse-angled triangle, and they are always much smaller than the succeeding branch leaves; the margin of narrow cells which borders these leaves is widest at the base of the longest side. The peduncular leaves, or those found at the base of the naked branch which bears the fruit, differ from the others both in form and structure, sufficiently to render their description necessary.

As an aid to our examination of leaf structure, certain colouring agents are of advantage in enabling us to obtain a better definition of the delicate textures of which the leaves are composed. Iodine and sulphuric acid or a solution of biniodide of zinc, have been used for this purpose, the latter of these being most convenient, an immersion of the leaf for two to twenty-four hours being required. Transverse sections of the leaves are also necessary in order to determine the relative positions of the chlorophyllose and hyaline cells; these are best prepared by immersing a branch in thick mucilage of gum arabic, and when sufficiently dry, enclosing between two pieces of elder-pith, and slices of the whole cut and placed in water.

Anatomy of the Leaf.—Hedwig, in his 'Fundam. Hist. Nat. Musc.,' i., p. 25 (1782), evidently noticed the composite character of the Sphagnum leaf, for he mentions the large areolæ, void of chlorophyll, traversed by very fine vessels, running double, which he thinks may possibly correspond to the ducts of flowering plants, and these anastomosing vessels containing parenchyma. Moldenhawer first pointed out the true nature of the two kinds of cells, and the presence of threads and pores in the vesicular cells, and Von Mohl afterwards confirmed his views and elaborated the whole organization of the Sphagna. A Sphagnum leaf consists of a single

stratum of cells, the framework of which is constituted by network of extremely slender coloured or chlorophyll cells, into each of the meshes of which we might fancy one of the vesicular cells had been dropped. By section we see that the relative position of these two kinds of cells to each other may vary, for the chlorophyll cells may lie midway between the anterior and posterior surface of the leaf, and their section shows us that they are lenticularly compressed, or they may take part in forming the anterior or posterior surface of the leaf, their transverse section being triangular, so that they resemble a wedge pushed in between each pair of hyaline cells: minute as this structure is, we must admit its importance, since it originates in the fundamental formation of the leaf.

The hyaline cells are more or less united by their adjacent walls, and nearly always contain threads attached to their internal walls; these threads may form complete spirals, composed of one or several fibrils, or they may be broken up into rings and spiral fragments, and sometimes run across diagonally so as to unite two spirals. Threads, however, are not always present in all the leaves, for in *S. fimbriatum* they are wanting in both the stem and peduncular leaves, and others have them in one part of the leaf while they are absent from the rest; in *S.* (*Isocladus*) *macrophyllum* no threads are seen except those forming a ring round the orifices of the pores. The threads are firm and intimately united to the inner wall of the cells, so that in *S. subsecundum*, the walls of the hyaline cells are strongly contracted by them.

The apertures or pores are most abundant on the back of the leaf, and stand near the adjoining cell-walls; they vary in size and number according to the species, and no doubt originate by the resorption of the delicate cell-wall, within the boundary of a small thread-ring. Besides these, Russow calls attention to larger openings which become visible after treatment with iodine, and indicating more extensive resorption of the cell-membrane. Thus in the lower part of a branch leaf of *S. fimbriatum* so treated, these large apertures reach across the whole width of the cell, and stand between each pair of thread spirals; in the corresponding leaves of the nearly allied *S. Girgensohnii* this resorption appearance does not occur. In leaves from the pendent branches of *S. intermedium*, a hole is always seen at the apical end of each cell. In *S. Lindbergii*, *fimbriatum*, and *Girgensohnii*, whose stem leaves are fringed at the apex, this appearance is due to complete resorption of the membrane of the hyaline cells, and consequent projection of the intermediate parenchym cells.

The chlorophyll cells of peduncular leaves usually have deficiencies in the thickening layers of their walls, and these standing opposite to each other, resemble imperforate dots, not unlike the dotted pleurenchyma of coniferous wood; a similar condition is

observable in the walls of young axile cells of the Sphagnum stem.*

In most peduncular leaves the hyaline cells are less evident than in those from other parts of the plant, and are often confined to the upper third of the leaf.

Development of the Plant.—To the investigations of Nägeli and Hofmeister are we principally indebted for an account of this interesting process. It not unfrequently happens that in floating Sphagnum plants, whose capsules are submerged, that the spores germinate in the capsule, where their delicate pro-embryos are so closely packed, that the whole contents are caked together into a solid mass, which first becomes free by the breaking up of the capsular wall, and in this condition swims about until the individual plantlets have separated from one another to establish themselves on some floating object and undergo further evolution.

The spores of capsules maturing out of the water, germinate on damp earth in two to three months. Prof. Schimper rarely noticed the pro-embryonal cell break through the exospore in less than five weeks.

In water the pro-embryonal cell elongates and ramifies as confervoid filaments formed of nearly globose cells, and the terminal or some other cell becomes the mother cell of the young plant, while the rest ramify and put forth brood-gemmæ, which develope into young plants, the radicles being always distinguishable by the *oblique* commissural walls of the cells.

The spores germinating on damp earth behave in quite a different manner, the pro-embryonal cell goes on subdividing in a horizontal plane, so that an expansion results resembling the prothallium of Equisetum, or the perfect plant of Blasia or Anthoceros. This hepaticine frond throws out radicles from the under surface and margins, and from these again brood-gemmæ sprout out, from which arise prothallia precisely resembling the first.

The first commencement of the young plant originates in a tuberculoid aggregation of cells, some of which develope downward into hair-like radicles, while the upper cell elongates and subdivides to form the young stemlet, some of the cells, laterally becoming free, form the first rudiment of leaves.

The young stem, at first transparent, soon acquires minute chlorophyll granules, and a differentiation into medullary, ligneous, and cortical layers is early set up. When it has reached a height of about 5 mm., it begins to throw off at the sides single flagellar branches, which arise laterally from the uppermost leaves, and are crowded together at the top of the stem. The branches come off at every fourth leaf, as an obtuse bud of few cells, on

* Hofmeister, 'Higher Cryptogamia,' pl. xvii., fig. 9 *b*.

which, when three cells high, leaves also form, and division into branches takes place. The growing point of the stem is conical, its terminal cell apparently subdividing in five directions, and thus continually elongating the stem; by longitudinal division and transverse extension of newly-formed cells the terminal cone thickens itself from above downward, and the base, constantly forming anew, attains the diameter of the already completed stem.

The rudimentary leaves are arranged in five rows on the young stemlet; the mother cell of the leaf acquires its first segmentation by a septum springing laterally from the longitudinal axis and perpendicular to the surface, the apical cell again dividing by a septum in the opposite direction, meeting the first at an angle of 90°, and by repeated division of the apical cell the leaf extends, and we have a simple areolation of large quadrate cells, those at the margin being more elongated, and all filled with a slimy fluid, in which float small pale-green chlorophyll granules, chiefly grouped around the large nucleus. With the formation of the fifth leaflet, however, begins that regular differentiation into two constant cell forms, which gives to the Sphagnum leaf its peculiar character; each of the square cells divides unequally by a septum parallel to one of its walls, the larger portion is then divided, by a septum parallel to the narrow sides, into two unequal cells, the larger square, the others elongated; and the leaf now consists of a system of square cells, each of which is surrounded by four oblong cells. In the latter, chlorophyll granules rapidly increase in number and size, while the pale-green mucilage filling the larger square cells disappears, and their contents become clear as water. The prosenchym cells extend themselves more and more at the expense of their protoplasm, and receive fibres on the interior of their walls, which at first are only fragments of rings, but afterwards run together into complete rings or spirals; finally, also, small scattered rings appear on the internal surface, which become thickened by resorption of the included disk, and form the margins of circular foramina. In the young leaves the central cells multiply and extend themselves after those at the margin have ceased to do so, hence the young leaf acquires a cucullate or hooded form; the process still continuing, the hood becomes split and the leaf flattens out, but the apex bears evidence of this splitting in the lacerated or strongly-toothed margin.

The male flowers in Sphagnums are arranged in amentula, which occupy the termination of a certain number of the divergent branches, and under their covering leaves, which are somewhat broader and more coloured, are concealed the antheridia. Frequently these branches do not terminate with the flower catkin, but continue their growth with the ordinary leaves, and in *S. rigidum* and *Lindbergii* the flowering branch elongates and

hangs downward like the pendent branches. Each male flower of the catkin consists of one archegonium, which stands laterally to the supporting bract, to which it bears precisely the same relation as a branch fascicle does to its stem leaf. The covering leaves are usually more closely imbricated than the branch leaves, and in *S. acutifolium* are of a fine carmine colour; in *S. cuspidatum*, ochreous; in *S. fimbriatum*, lively green; in *S. subsecundum*, olive green, &c.

The perigynium or sheath of the female flower is readily recognized by its long conical form and deep-green colour, and stands on one of the short lateral branches of the capitulum; the inner leaves are much elongated, and enclose one to four archegonia, only one of which, however, developes into fruit. As the pseudopodium, or peduncle of the fruit receptacle elongates, it usually happens that the leaves it supports are also drawn farther apart.

In the young capsule the sporangium only reaches a little way below its middle, the rest of the cavity being filled with a soft pale-green cell-mass; in the ripe capsule, on the contrary, we find nearly the whole internal space empty, the columella has broken away from the vault of the sporangium, and along with the cellular mass has shrivelled back to the base of it, but the sporangium, firmly cohering with the lining stripped from the inner wall of the capsule, is left hanging in its upper orifice, where it stays, until by contraction of the capsule the lid is forced off with a little explosion, by which the contents are expelled. The outer wall we find consists of cells forming longish hexagonal meshes, presenting a nodule at each angle, brittle, thickened, and yellow-brown in colour, scattered among which are numerous small stomata.

Frequently the lid remains fixed at one point to the rim of the capsule, which it closes again when moistened, moving as if on a hinge; and when the fruit remains under water, the lid often does not open, but the capsule, with its contents, falls away from the vaginula, and as the columella decays, the spores escape through the aperture, or if they have already begun to germinate, their expansion forces off the lid also, leaving the old capsule wall with a large round opening at each pole, one corresponding to the lid, the other to the insertion of the pedicel, and such frequently come into the field of our microscope when operating on tufts of Sphagnum.

IV.—*An Instrument for Micro-ruling on Glass and Steel.*

By J. F. Stanistreet.

Plate CVI.

[Although an illustration in the form of a woodcut of this ingenious little machine has appeared elsewhere, we have thought it worth while for the benefit of our readers to reproduce a plate of the apparatus on a larger scale and in clearer type. The adjacent Plate is copied from a photograph of the machine.—Ed. 'M. M. J.']

To the 'English Mechanic' Mr. Stanistreet sent the following description, as it was in its pages that he first saw a reprint of Mr. Slack's paper, which appeared in this Journal some months ago, "On Optical Appearances of Cut Lines in Glass." Mr. Stanistreet says:—

It may encourage your more able readers to know that my lathe and tools, as well as my experience in using them, have been all self-acquired within the last two or three years, during my confinement to the house as a permanent invalid, and that I never

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EXPLANATION OF PLATE CVI.

Fig. 1.—Diamond, set at an angle of 60°.

„ 2.—Mandril, to which the disk to be ruled is cemented.

„ 3.—Worm-wheel of 250 teeth worked by endless screw—the wheel is graduated with index, and has spring stops for one-half or one-fourth of a revolution, thus dividing the periphery of the circle into 1000 parts, or to 0° 21′ 36″.

„ 4.—Leading screw of 100 threads per inch.

„ 5.—Winch turning the leading screw, with spring stops for subdividing to the $\frac{1}{10000}$th of an inch, if a diamond fine enough for such a scale should ever be obtained.

„ 6.—Spring made from pianoforte wire.

„ 7.—Pressure spring working on glass roller.

„ 8, 9, 10.—Springs for delicate adjustment of pressure—one above, one below, and one lateral.

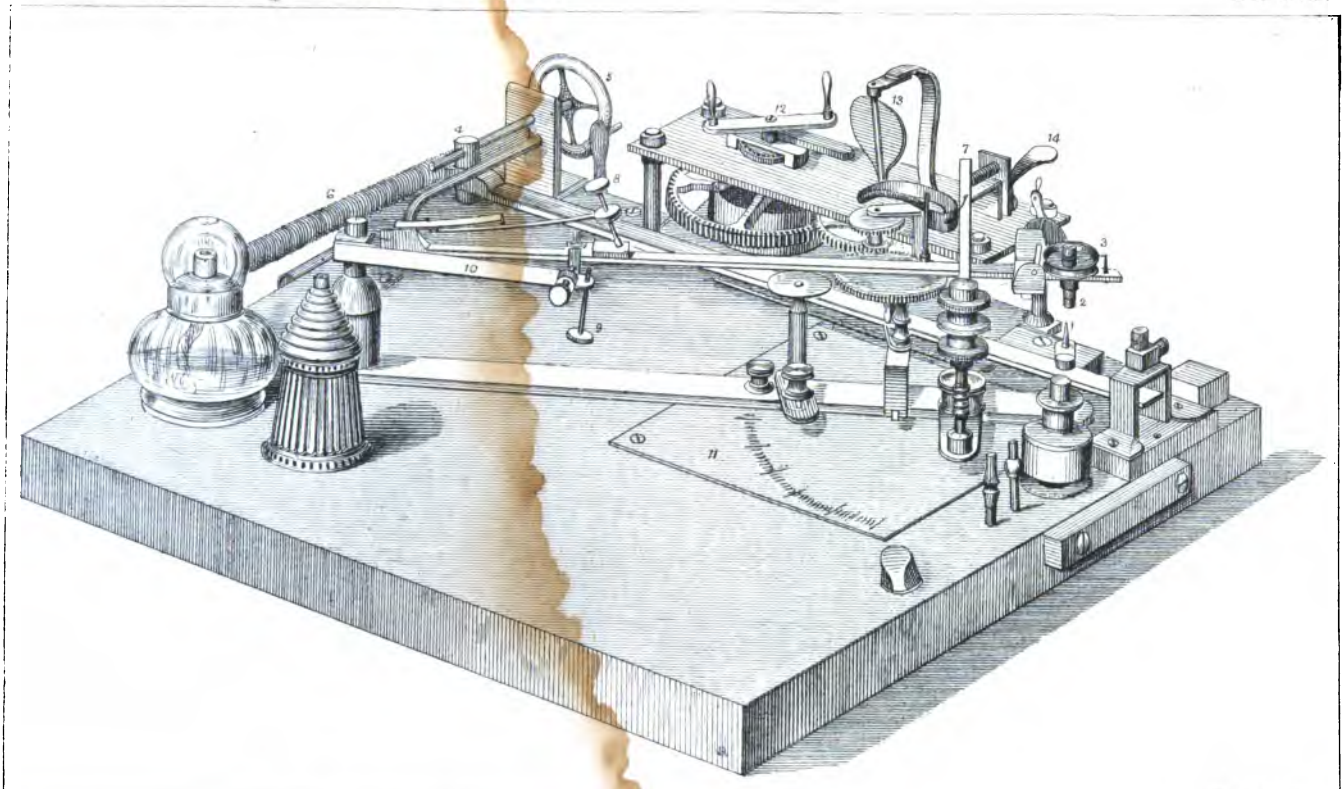
„ 11.—Graduated arc of circle from 0° to 90°, giving divisions from the $\frac{1}{1000}$th to the $\frac{1}{10000}$th of an inch apart, in the ratio of the cosine of the angle at which the ruling bar is set. When placed at 0° it rules lines the $\frac{1}{1000}$th of an inch apart. The machine when photographed was set at an angle of 60° for ruling 2000 lines per inch (the cosine of 60° being 0·5, or one-half of the radius). To rule 10,000 lines per inch it is placed at an angle of 84° 15′ 39″ (the cosine of which is 0·10, or one-tenth of the radius).

„ 12.—Double-handled winch for winding up the self-acting machinery. This consists of a train of wheels and pinions driven by the spring of a musical box, and ending in a fly, which regulates and controls the rotation of the cam plate. The fly makes 3840 revolutions for each turn of the spring wheel; and the machine will rule more than 1000 lines without being re-wound. Since the photograph was done an addition has been made of a wheel and index for recording the number of lines actually ruled.

„ 13.—Fly, regulating the rotation of the cam plate.

„ 14.—Trigger and springs for starting the machine.

[Note.—The spirit lamp, match box, and oil bottle, &c., ought to have been removed; but being let into the stand of the machine they have been copied by the photographer.]



Mr. Stanistreet's machine for micro-ruling on glass and steel.

had a lesson in mechanics. Every part of my machine (even to the screws and smallest details) has been made by me without assistance. I hope, therefore, some of your readers more favourably circumstanced, may improve upon my first trials, and far excel me in this beautiful and interesting art.

My machine was originally planned for ruling micrometer scales for use with microscopes and telescopes, and its application to ruling diffraction patterns (however beautiful) was quite a secondary object with me, but Mr. Slack has, by his valuable researches and papers, invested these ruled patterns with a practical value and interest which I had not originally attached to them, and as a microscopist I appreciate very highly the teachings conveyed by his papers, showing the necessity of educating the eye and judgment to enable the observer to interpret and correct the illusory appearances so constantly met with in working under the higher powers of the microscope.

The Plate shows the machine with the self-acting machinery added since Mr. Slack's paper. "This self-acting machinery consists of a spring (from a musical box) driving a train of wheels and pinions, ending in a fly, which regulates and controls the rotation of the cam described by Mr. Slack, all the wheels and pinions being (like the machine) made by my own hands, except one little steel pinion which I purchased from a watchmaker."

The machine is constructed for ruling lines from $\frac{1}{1000}$th to the $\frac{1}{10000}$th of an inch apart, and I have added to it the means of further subdivision to the $\frac{1}{100000}$th of an inch; but I have not yet been able to procure any diamond fine enough for ruling distinctly more than about 5000 lines per inch.

V.—*On the Conjugation of Amœba.* By J. G. Tatem, Esq.

I wish to recall to the recollection of the members a paper brought before them in a previous year,* "On Free-swimming Amœbæ," of which two species were described, but to which, under the impression that they presented phases only of Amœba life no specific names were assigned. I then stated "that we had no further knowledge of Amœba propagation and reproduction than that by fission. An over-extended pseudopodum, perhaps larger than common, remains attached to the spot to which it has been projected, separates from the parent mass and creeps off as an independent living creature," and that this summary and somewhat rude process, actively as it might be carried on, could only account for the presence of a vast number of individuals within a limited

* Vide 'M. M. J.,' June, 1869.

space, and not for their dispersion over an extended area; and ventured the assertion, though no evidence was then forthcoming in support, "that those large Amœbæ so frequently met with in the autumn months are actually the incorporation of two individuals in a copulative act," from which free-swimming ciliated germs might eventually issue. Since that communication was made to the Society, I have had an opportunity of obtaining strong corroborative, if not entirely convincing, proof that that which was then conjecturally advanced, is absolutely true as to the fact of conjugation, and I submit to your inspection a diagram of a large

Amœba villosa. *a*, *a*, pseudopoda.

Amœba villosa which presents the appearance of the semi-union of two individuals, both externally and internally, in the circulation of their granules and food contents, at the moment preceding complete mutual interpenetration. Moving rapidly by broad, rounded pseudopoda thrown out from either side of the broader rounded anterior portion—though never crossing a median line, clearly defined by the circulating particles flowing in parallel lines within, as indicated by the direction of the arrows in the diagram, no other impression could be received than that a semi-conjugated condition here presented itself. Unhappily its disappearance under a mass of dirt, from which it never again emerged, precluded the possibility of determining whether or not the amalgamation would in time have become more perfect, and whether the sarcode masses of the two would have so completely intermingled that it would have ultimately borne the aspect of one large but ordinary Amœba. Assuming this to be an instance of true Amœba conjugation, can

we doubt that such a reproductive process must eventuate in the evolution of some kind of germ? but what such may be it would be idle to speculate. I desire only to put the observed fact upon record, and to engage the efforts of our members in the elucidation of that most recondite, though most interesting subject—the reproduction and distribution of our fresh-water *Rhizopoda*.

VI.—*Crystallization of Metals by Electricity under the Microscope.*

By Philip Braham, Esq.

Plate CVII.

During some years of scientific investigation I have been in the habit of submitting every eligible experiment to microscopic examination, and during an investigation concerning a metal tried the effects of electricity on it in connection with an acid solution. I was delighted to find a brilliant crystallization start into life, but shortly becoming dull. Following up that experiment by using neutral salts, and terminals of the base, have succeeded in crystallizing gold, silver, copper, tin, zinc, and lead, and have every reason to believe I shall be able ere long to crystallize every other metal.

The instrument used I give a sketch of, and the electrical stage (a diminutive double discharger, which does excellently for spectrum analysis in connection with an induction coil) and galvanic battery, with fittings for varying the quantity and intensity of the electric current. The sketch of the discharger shows the manner in which it is fitted. The two short pillars A A are of ivory, turned to a cup-shape on the top to fit the balls; the springs B B keep the balls in their place, and are fastened to the side of the ivory pillars by the binding screws C C, to which the wires from the poles of the battery are connected; the ends of the discharger rods are split to receive the short wires of the metal under electrolysis.

The battery is similar in its construction to Smee's, but with carbon plates instead of platinized silver, and excited with bichromate of potash and sulphuric acid, it has the advantage of not evolving fumes or acid spray. The wheel at the end D is provided with a break E, to keep the plates at any required depth in the solution. The spring clip G connects any number in the series of six cells. The solutions of the salts should be as strong as possible, without a tendency to crystallize.

A drop of the liquid is placed on a microscopic slip, and the ends of the wires fitted into the discharge rods dipped into it, and kept apart not more than a tenth of an inch, lowering the battery into the solution, and carefully watching the terminal in connection with the zinc pole: the instant action is observed clamp the wheel,

and watch the result. Practice will show the best power of quantity and intensity to use.

The use of a drop of liquid necessitates the microscope being in a vertical position: to obviate this inconvenience I have adapted a rectangular prism, and turned the eye-piece part of the tube horizontally, as in Amici's, and also made the prism to revolve concentric with the optic axis of the instrument, which enables several persons sitting round the table to see the experiments at any particular time by turning the eye-piece towards them.

The light must be entirely reflected. By placing coloured glasses in the diaphragm frame, and sending light through them from the mirror, a clear outline is given to the crystals, and an illuminated background, which produces an exceedingly beautiful effect.

I am still pursuing the investigation, and hope to publish further interesting results.

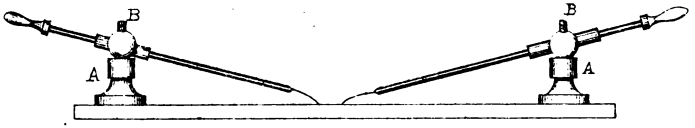
VII.—*Infusorial Circuit of Generations.*

By Theod. C. Hilgard.

(*Continued from p.* 233, *No. XXXV.*)

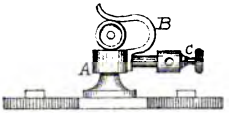
These "currant"-yolks enlarge in size, and soon at the (darkening) circlet or rim of the introversion reveal a rapid rotation and "ciliary motions," and still later, a contortion and volubility of contents really perplexing to the attentive beholder, who in vain attempts to determine its form, or at least to detect it in the moment of hatching, "anxiously wasting whole nights and half days" thereon, as Ehrenberg has expressed himself on a similar subject. At last the membrane bursts and extrudes a globe or halo of gelatine, containing a crucible-shaped body, gently moving, which, when finally set free by the rupture of that gelatinous halo, at once elastically *extruding the inverted part*, takes a shape resembling a rice-palea or the fore-wing of a thunder-fly (Thrips), travelling broad-end foremost with great velocity and steady as an arrow. After a while a somewhat ludicrous scene ensues, when the little animal, by shedding its fissured skin or scabbard, is seen violently struggling to disentangle its large jerking bristles hidden in the *veins* of the sheath, and its small body. It thus appears like a little dwarf, frantically floundering about in a Spanish cloak, spurs, and sword too large for their owner. It now represents a very small Oxytricha with comparatively very long, stout, but as yet softish, bristles.

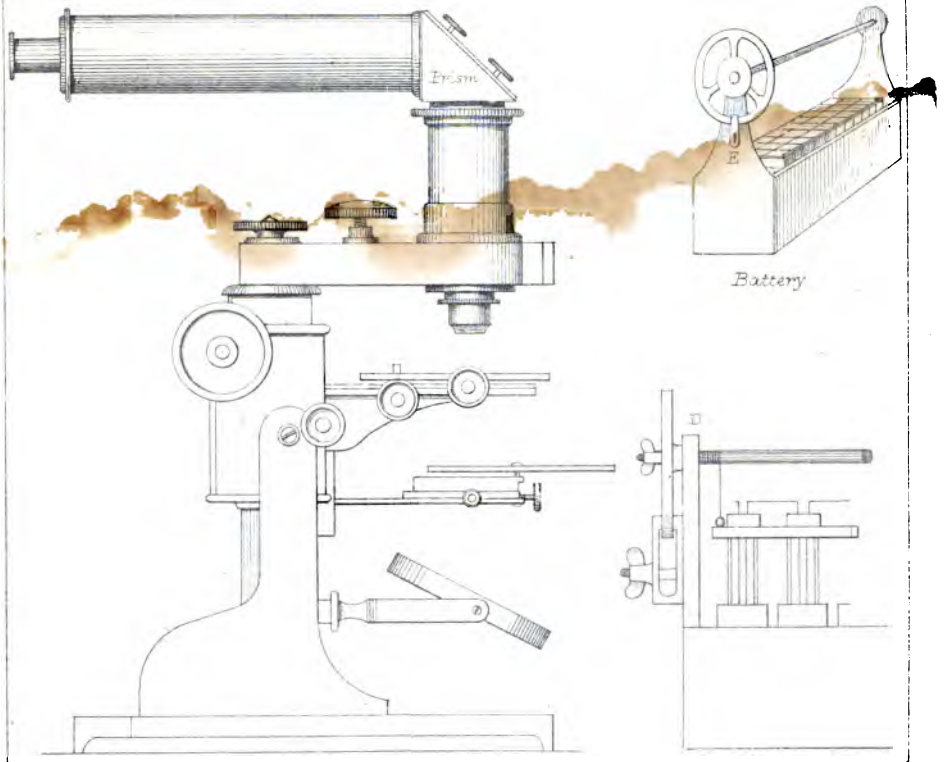
This formed the more *direct* evolution from the Oxytricha pellet, *viz.* out of its circular "currant-vesicles." Its enveloping



Discharger

Spring Clip





Battery

Philip Braham del

Mr Braham's Electro-microscope

grumose mass of "trabeculated albumen," however, keeps still increasing to the appearance of a loose snow-ball as it were, and each single trabecular joint assuming a sort of warped §-form and a jerking spasmodic commotion, they at last tear loose singly, and escape each as a lanceolate, *warped* and finely-tailed "*Vibrio Termo* Dujard."* In consequence of its twisted shape, it makes its way with a vacillating archimedian motion, being constantly turned round as it is rushing onward. When about $\frac{1}{200}$ line long, it already clearly reveals the (still warped, but finally flat) *wafer*-shaped body; and the longitudinal *striæ, fringed with an undulating fleece,* as well as the oblique, ciliate mouth, which also characterize its later stages. From an oblong orbicular pouch-shape, when about $\frac{1}{150}$ of a line, it becomes round like "navy"-beans (up to $\frac{1}{100}$ of a line) only a little tapering at the upper end; the small oblique mouth being a little above the middle. The delicate longitudinal striæ all over the body—melon-fashion—give them an iridescent appearance, both under the microscope, singly, and when swarming in masses on the surface, *e. g.* of aquaria, or of the draining-pans of flower-stands. The striæ are apparently set with very soft *undulating threads,* resembling wool, nearly half a diameter long, in likeness of "ginned" cotton-seed. This feature is absolutely overlooked in most of the figures from Ehrenberg up to the present day; otherwise the former's "*Paramecium kolpoda*"† would seem to represent a few of its onward developments.

The body now commences to *bisect,* at first crosswise; becoming *waisted, across the mouth,* so that each half has a part of the old one. After assuming the form of an 8, they, after long struggling and toiling, bisect, often spinning out a long gelatinous thread (as of a limpid gum) and jerking each other most lustily; but after disruption they presently round off.

In this condition, and the following, the bodies contain one larger and a great many smaller granular *pellets,*—"yolks" or "germinal specks," which I have not distinctly seen discharged. But now the surface of the water becomes clouded with such granular balls, of uniform molecules (about $\frac{1}{1500}$ line in thickness) that likewise germinate into the fragiform clouds, alluded to in connection with Vorticella, &c., and is covered with an apparently amorphous, most delicate but cohesive pellicle (as of collodion) at the superficial contact with air. All these forms, as above stated, when caught on

* The name of "*termo*" (*τερμων*, a boundary-pole or stake) probably referred, originally, rather to the cylindric "battering-rams," extruded from diffluent "currant"-vesicles (or *amœba*) of the paramecium cloud-dissolution, as below detailed. The albuminous Oxytricha-pellet is pretty well represented in A. Pritchard's 'A History of Infusoria,' tab. xviii., fig. 69. The indistinct §-shaped (constituent or) developing particles, however, are there technically represented by shading with cross-striæ, conveying a false impression of their shape and structure.

† 'Abhandl. Berlin Acad. Wiss.,' 1834, tab. iii., fig. 3.

a dry surface (*e. g.* by their undulating floss), instead of forming into a dry scab, suddenly become *liquid* (like fusing lead), with an immense internal commotion of parts, and bodily dissolve into such cloud-molecules. The "wool" itself becomes *quasi*-"dropsical," and each single fibril diffluent into a series of such uniform globular molecules, which at first are endowed with an independent motion, vibrio-like. Besides this, most of the encystments, moultings and yolk-extrusions take place under the isolating cover of that uniform protoplasm-membrane, which seems to exale a sort of bituminous odour (like the fumes of burning flesh, sun-baked carrion, or the rank smell of miry river banks). Membranes, as thin but chemically homogeneous organic substances, being impermeable to certain gases, while permeable to others, a good deal of physiological interest is involved in the study of this protoplasm-membrane, and its relation to the swamp-gases. The particles of the nubecula are uniformly globular.

After repeated *cross*-segmentations, these undulate fimbriate bodies, always revolving about the long axis (while evidently travelling onward by the action of the *ciliate mouth*) divide *lengthwise*, from below upward; thereby becoming somewhat purse or tear-shaped; the mouth being split in two, so that both stand "plying" mouth to mouth, while yet connected at their foreheads, as it were. These finally tear asunder by indentures, after which each has the shape of a crooked glass-tear. When more adult, and about $\frac{1}{40}$ of a line long, the internal yolks and designs have disappeared; the sarcode assumes a uniform yellowish tinge; its mouth forms deep cavities, while its front is toppling over like the hood of an Indian turnip (*Arum triphyllum*) or of a Sarracenia leaf. It now contracts to a globe and encysts. When a smooth, transparent crust is formed, gradually an inward gyration of cilia (as of an enclosed centipede), which ultimately becomes very violent, is observable; and at last the excessive fatigue of watching this tantalizing gyration may be rewarded by seeing the *inmate emerge*, either as quite a large but excessively limber, fluttering and transparent, full-size single Oxytricha; or else several smaller, mostly narrow, triangular slips* escape, with the same exceedingly restless volubility; the marginal bristles not yet being stiffly extended in a plane, but ruffled up and down like the bristles on the undulating borders of a thistle-leaf. As they feed and the tissues become scatent, the entire form of an Oxytricha is presently acquired.

I have observed still another development of Oxytricha; its first source, however, being as yet unknown to me. There appear on the field of action numbers of quaint-looking, big-eyed balls, about $\frac{1}{100}$ line thick, snouted, as it were, with a sort of "hair-lip" resembling a duck's bill; the stiff bristles within the bill-shaped

* The figures L and M, p. 447, in Carp. 'Micr.,' seem to belong here.

mouth quivering with a sort of expressive smirk, and looking altogether odd.

They come full-sized and booming upon the stage, and in this respect argue a direct derivation from certain haw-shaped, five-costate vorticellan buds, with a contracted *pappus* of stiffened cilia around the orifice, spinning and rebounding like humming-tops. The "goggle" now soon becomes stationary, and shortly after, rapidly expanding, and its germinal speck or nucleus (the "eye") particularly enlarging, within half an hour it dropsically flattens out into a pretty well-sized Oxytricha,* by a similar sort of internal fluxile commotion of particles as when the animals dissolve into molecular "sauce."

But Oxytricha is not a perfect animal. It has no membranes, and evidently no fibrous tissues at all. The entire texture apparently remains in an embryonic, *vitelline* condition, as yet.

I have in a single instance witnessed what appeared to be the *moulting* of a perfect Oxytricha. The front border was somewhat removed from the body, which it crowned like the crest of an ancient helmet, and within each rigid bristle ("style"), *as within the fingers of a glove,* was contained the far more delicate corresponding one of a clear (and now entirely yolkless-bodied) animal, the lower quarter being in a like manner hidden in a part of the old coat. I thought it was plainly identical with the following animal, whose development brings us up to "*Paramecium Aurelia.*" As I have not been able to chance upon such a moulting process again, I reserve the decision.

A clear (internal) animal is apparently developed by this moulting of the Oxytricha. Of the latter, the very bristles, when detached, seem to possess individual vitality, singly beating about for quite a while, and even empty coats (apparently *shed*) sometimes behave as if they had a life of their own. At all events, at a certain epoch there appears at once the next form in question,† in full size upon the field; the transparently clear bodies sometimes showing a scalloped border, and alveoli, as of *former* yolks, *extruded* —that soon smooth over. In outline, the animal appears somewhat like the soft parts of an oyster, being flat, somewhat lop-sided, in the shape of a human ear—tip foremost. It is "doubled up" at the straight border, the broader lower rim being overlapped, *as by a lid,* with a smaller, but thicker, upper flap ("lorica") containing one clear germinal speck. This animal *opens like a book,* undoubling its flaps; and it is thus that it devours its prey (such as

* This somewhat resembles fig. F, turning, by fluid expansion, into fig. E (Carp. 'Micr.,' *ibid.*); fig. F, however, requiring to be duck-billed, as it were, and fig. E to be lop-sided and the nucleus more central.

† Perhaps the "*Euplotes*" of authors. Their descriptions and figures, however, offer nothing that sufficiently resembles this very common form, so as to be readily identifiable.

conferval spawns, &c.), by bodily enveloping them like a ray-fish (*Raya*), enfolding the nourishment as if *fused around it*, and the whole surface exhibiting an incredibly rapid ciliary commotion *during the whole process of digestion*. This done, the cloak again unfolds, often appearing like two stipules, *e. g.* of a *Liriodendron*, and then closes up again. On drying up, or in search for air and *moisture*, the animals are often seen to mutually enfold each other's flaps. This cannot, however, be interpreted as a sexual copulation, seeing that in the first place they neither develop any eggs, nor in the second place do they even extrude yolks; but their onward development is *by encystment*.

Within a few minutes such a full-grown "oyster-grub" is seen contracting its big flap, so as to present the shape of a hat with a warped rim and hemispherical crown, the latter formed by the blunter lobe, which contains the "speck" or "eye," and, contracting, gets hemispherically rounded. Very soon (with a constant adjustive quivering of the cilia-like bristles) the whole is rounded into a globe, wherein the doubled inside forms a ciliate hiatus. The latter, soon contracting, closes over. Nothing is now seen but a ball with a clear "germinal speck." In a few hours a *double* contour (the outer one granular) is exuded. The speck or "eye" itself now becomes dusky and granular. It increases. It bisects "Gregarina"-fashion. Each pear-shaped segment again acquires a clear speck or "eye." They elongate, being connected by the blunt ends,—each one tapering to a very soft apex; and these very large germs or pseudo-Gregarinas at last become liberated, probably as "*Paramecium Aurelia*," which now appears *full-grown* on the scene.*

It is about the length of the Oxytricha, about three times the length of the revolving wool-fringed grubs of the Oxytricha, and by all means more complexly organized than either. It has the shape of the (shoemaker's) *last* for a very elegant lady's shoe. From one side it therefore gives the figure as of a foot-print (without the toes); but viewed on edge has a pointed rear end, and in this profile it "takes the name" of Paramecium *caudatum!* The ankle of that "last," however, is bevelled away, leaving the instep a ridge. Its oral aperture, not clearly distinguishable, is in the middle, slanting almost longitudinally for about one quarter of the length of the body. It seems to work its way, dashing by vacuole-contraction, while at the same time revolving by a roundabout coat or film of short pubescence, almost too delicate to be made distinct. In what appears to be the abdomen it has the well-known circular *pulsatory vesicle*, wherewith it propels itself, and around which point it is often seen spinning like a wheel. A system of fusiform

* The developmental experiments were made in small parcels, forming a drop (between glass slips, somewhat held apart) and preserved from exsiccation.

or bulbous vessels radiating around the pulsatory vesicle, contract as the vesicle expands, and *vice versâ*, as is well known; and some seem to have several such *pulsatory* "vacuoles." The body is turgid with rather small germinal yolks. These animals I have never seen bisecting either lengthwise* or across, nor copulating sexually. The latter, however, seems to take place with the Planariæ, which also show the staghorn-shaped entrails analogous to those of the marine (true) Planariæ and joints of the tape-worm, whose detached individuals are also known to hover freely in a liquid, like these Ciliata. It is supposable that the large Paramecium, with pulsatory organs, is the young Planaria; but it is certainly not itself an adult body.†

The further and most remarkable of all these progressive and retrograde developments is the following. The well-fed and full-grown but *entrailless* Paramecium Aurelia becomes slow and lazy, greyish with the teeming germinal contents, and in a few hours may be seen motionless as the fabulous "Kraken" of ancient Norway. Its *entire substance* now commences swelling forth into compact, fragiform "germinal clouds," while a great many of the germinal specks, now become less obscured, are plainly discernible as of the clear "*currant-shaped yolks*" kind. These in a short time, however, commence *moving*, and while some of the smaller ones are being propelled by adherent motile granules (probably the "Acineta" *Auct.*), the larger ones move by contraction, *viz.* their "circlet" becoming *everted*, they now *crawl forth*, like a very limpid *grub*—resembling a sort of *tumbling sac!* This "tumbling" is produced by the most marvellous facility it possesses of protruding long, blunt branches (like little stove-pipes) on *any* part of its surface by eversion, so that in a few moments its form is entirely changed. Its contents are a visibly and rapidly circulating *so-called* "rotating protoplasm," composed of mostly very transparent *individual vibrionic particles*, partly bulky, but mostly very small. Some dark (red or brownish) vibrionic dots are also discernible.

It now takes the form which has been called "Amœba." This form, however, likewise occurs when similar yolks or "acinetæ" are expelled from *vorticellan* bodies. In either case the "tumbling sac" lastly attains a versatile-campanulate star-shape *with "pseudopodia,"* from which break forth volumes of minimal vibrios, and quite large, cylindric bits of rods, or (*pseudo*)-"bacteria." The latter here are thicker than fungine bacteria, and are neither coated nor ellipsoidally shuttle-shaped, but bluntly cylindric, like cartridges or butting rams. They possess a very forcible automatous motion, and like to congregate, and with great violence keep butting all together, one against the other, in a heap; and within a few

* Ehrenberg's figures, however, show it in that process (if not a mistake).

† Pritchard, &c., figure the planaria-like form as "adult Paramecium."

minutes the whole appearance has dissolved and passed into a "germinal cloud" of molecular "vibrionic" cell-life.

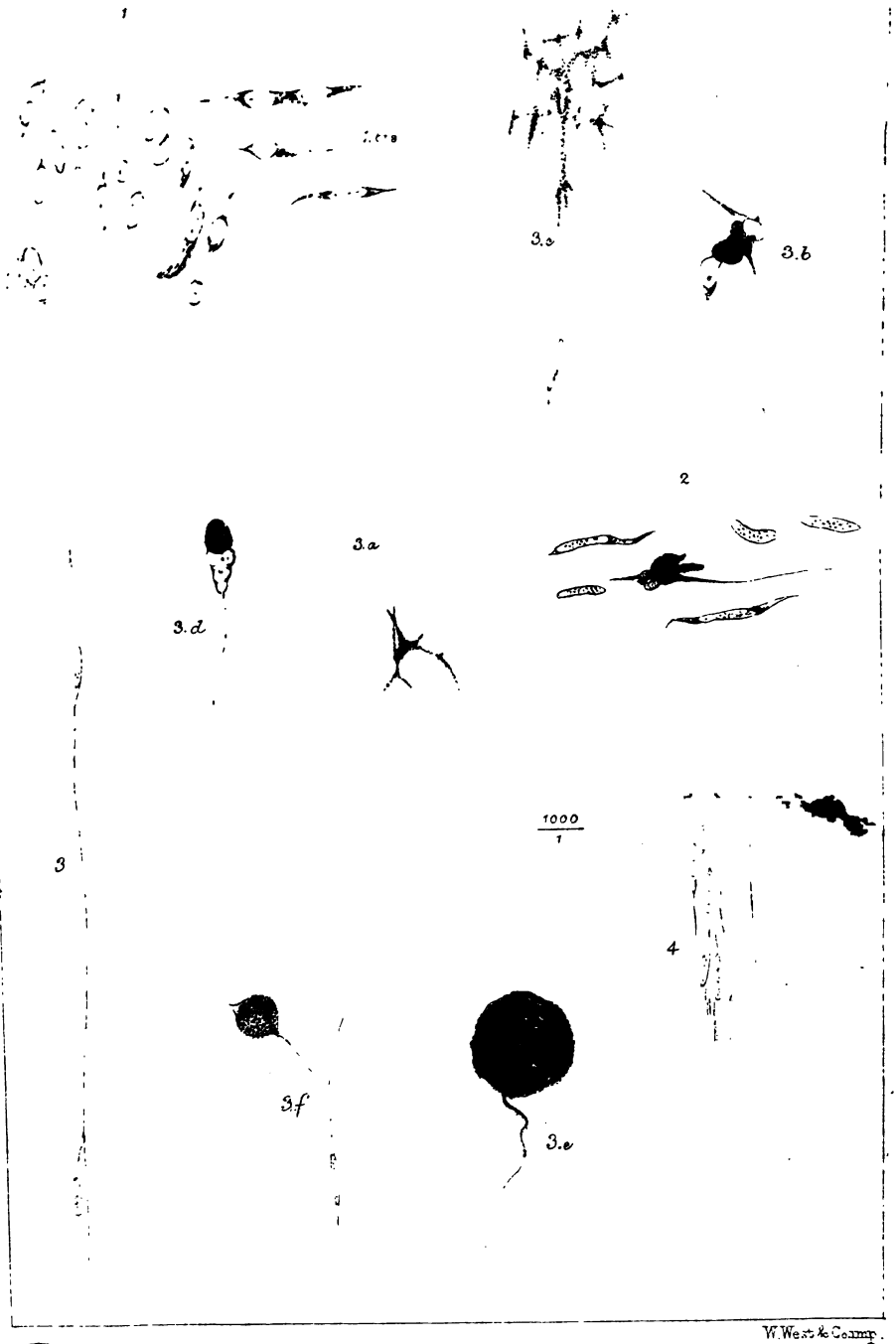
Besides the above circuits of generations, which probably comprise both the pulsatory Paramecians proper (Aurelia) and the Vorticello Oxytrichans (through the mediation of the "oyster" or "porte-monnaie-grub"), there occur frequently some *analogous* forms, such as "Kerona" and "Trachelium."

The last form of all to appear in infusions, &c., seems to be the well-known *Rotifer*, the developments whereof are perhaps related to some of those above detailed. It is, however, most probable, according to the observations of Prof. L. Agassiz,* who saw forms resembling the undulate-fleeced ("Paramecium kolpoda") grubs *bred from the eggs of Planariæ*,† that such are the adult forms (*if* adult). I have only occasionally met these swelled and pear-shaped dusky bodies, travelling both back and forward with equal facility, and remarkable for the staghorn-like designs of their *entrails*, thereby evincing something like a membrane in their organization, but the organ being itself of a sort of glandular structure. They are also said to bisect, like the Oxytricha. Some *young* ostensibly planarian forms, larger than Paramecium Aurelia, blackish, and shaped like a short broad lancet blade, which I have seen "bisecting," did so only while *encysted*, rotating in the manner of millstones, and the escaping animals had as yet no trace of the visceral organization, as found in the adult Planariæ (and also observably developed in Rotifer).

We are therefore still in doubt as to the true ultimate genus and species, and therefore have to *suspend* classification; the points of interest here submitted being the important physiological processes and transformations on the one hand, and the fallacy of foregone diagnostic terminology on the other. The description of the genetic phenomena of the so-called Fresh-water Algæ in their *unbroken continuity of developments*, as experimentally ascertained, I reserve for a future paper.—*Silliman's American Journal, August*, 1871.

* 'Ann. Nat. Hist.,' vol. ii., 1850, p. 157.

† This is no doubt what authors figure and describe as the "*adult* Paramecium Aurelia," with its staghorn-shaped intestines and swelled bodies. I am also under the impression that it was *this* form which I had formerly frequently observed in what appeared to be spontaneous *coitus*.



Color-organs of fishes.

VIII.—*On the Connection of Nerves and Chromoblasts.*

By M. Georges Pouchet.

Plate CVIII.

Since we have shown by direct experiments the influence of the nerves upon the pigment cells of the skin of fishes, and especially of the skin of flat-fish (*Pleuronectes maximus*, Linn.), the relations between these elements and the nerves naturally attract our attention. Herr Kühne [untersuchungen über protoplasma] in 1864 figured the larger connections between the nervous tubules covered with myeline, and the connective cells of the cornea. But beyond that the identity of these cells and the pigment cells is not established further than the doubtful effect of a reagent used by Herr Kühne, nitrate of silver, it does not appear that the conclusions of his memoir have been definitely adopted by anatomists.

The pigmentary cells or chromoblasts are situated below the skin, which is represented in many fishes, as in the Batrachia, by a delicate hyaline membrane, about 19 to 20 Paris lines in thickness. It is beneath the membrane properly so-called that the chromoblasts are found mixed with other elements, which seem of a connective tissue-like character. The chromoblasts themselves are essentially composed of a mass of sarcodic substance (or protoplasm), usually surrounding a nucleus, but being able probably to subsist without it. In the midst of this sarcodic substance is deposited the colouring matter, which is consequently a true pigment. This colouring matter is of various tints, but generally it is, in the least refrangible half of the spectrum, yellow, orange, red, brown, and black. Sometimes this colouring matter is liquid, and forms—as one sees it in the embryos of crustacea—a coloured drop in a portion of the sarcodic substance near the nucleus. When it extends in its amœboid movements, it draws over it the coloured drops.

At other times, and indeed most frequently, the colouring matter is spread out in granulations throughout the mass of protoplasm,

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EXPLANATION OF PLATE CVIII.

Enlargement × 1000.

Fig. 1.—Laminous elements of the membrane of the pectoral fin of a turbot, 4 centimètres long. *In situ.*

„ 1 *bis*.—Three laminous elements isolated.

„ 2.—Elements of the same membrane at another point. *In situ.*

„ 3.—Nervous fibre.

„ 3 *a*.—Chromoblast without nucleus in contact with a nucleus of nervous fibre.

„ 3 *b*.—Chromoblast in continuity with a nervous fibre.

„ 3 *c*.—Idem.

„ 3 *d*.—Nervous fibre in continuity with a nucleus of a chromoblast.

„ 3 *e*.—Chromoblast in continuity with a nervous fibre.

„ 3 *f*.—Idem.

„ 4.—Nervous thread accompanying a capillary.

which draws them with it in its different movements. But it sometimes happens that the finest expansions in turning upon themselves leave these granulations in the midst of the surrounding tissues.

Our observations, made to investigate the relation between the nerves and the chromoblasts, have been carried out upon the pectoral fin of young flat-fish, about 4 *centimètres* long. The organ taken from the living fish is treated with dilute acetic acid, about 1 to 1½ per cent.; then very slightly tinted with carmine. The preparation thus made has been examined with an object-glass of 10 mm., of Nachet, which admits of one seeing through the whole thickness of the fin. In all cases it is treated with chloride of gold, feebly acidulated with acetic acid; it was placed under this reagent after having been in acetic acid a sufficient length of time to admit of the detachment of the epidermis.

The only elements of a connective nature that I have seen are nuclei, surrounded by a greater or lesser quantity of a hyaline substance, which appears resistant, and affects a bi-polar disposition of the two extremities of the great axis of the nucleus. These cells without a trace of apparent membrane nevertheless differ much; in fact, as much in relation of form as in that of size of the nucleus as well as the polar substance.

Fig. 2, Plate CVIII., represents exactly one region of the interposed membrane with two rays. The nuclei affect a distinctly parallel relation; they are narrow with regard to their length. The polar substance is there, and recalls the appearance of the *fusiform bodies* of the superior vertebræ. In other instances the same elements show themselves disposed in a quite different manner. They are at the same time more voluminous, more closely related, and they affect a parallel disposition. Their position is then ordinarily perpendicular to that of the rays. In some parts it is not rare to see a certain number of these elements affect a definite (*déterminée*) direction, whilst others have a perpendicular direction.

The nuclei of those contiguous conjunctive cells, as shown in Fig. 1, vary considerably in volume, like the elements themselves, almost from the single to the triple. They assume in preparations treated as we have directed, an ovoid form, and seem to exhibit in their interior the trace of a voluminous spherical nucleolus. The diameter of the nucleus may be estimated on an average at 6—7″; its volume is always proportional to the quantity of polar substance. Two nuclei are sometimes seen in these fusiform bodies, which seem to prove that they multiply by fission. We have represented an example, and also some aberrant forms in Fig. 1.

Capillaries are rare, and are sometimes accompanied by a thin nervous thread, Fig. 4. The latter is recognizable by narrow

nuclei, four or five times longer than wide. They are filled with very small black granulations. The outlines are ill-defined. Their substance fixes carmine with great energy. The nervous bundles of the fins present only a very small number of those fibres, but they may be followed when isolated, during a very long course, as shown in the Figure. They may be recognized in this case by their continuity, for the fineness of those elements is extreme, and at the same time, by the presence on their course of nuclei sometimes near and sometimes remote from one another. Sometimes the fibre seems to grow wider in order to contain the nucleus, and at other times the latter seems to be thrown off laterally, in a kind of floating membrane. It must not be forgotten that we speak here only of the specimen prepared according to our directions.

Without asserting here the existence of a *direct* relation between the nervous fibres and the chromoblasts, we will limit ourselves to a statement of the result of our researches. The question is a very difficult one to solve, owing to the nature of the sarcodic element, which is usually recognizable by its granulations, but is for that very reason impenetrable to the eye, so that all observation must be summed up in investigating the continuity of a nervous fibre, characterized either by its length, and the physical peculiarities of its substance, or better, by the presence of its nuclei, with the sarcodic substance, without any hope of tracing the nervous fibre into the midst of the contractile element.

An auxiliary, however, presented itself, which might help to determine this continuity. The granulations of pigment given up by the expansions of the chromoblast, may in their contraction become an excellent *point de repère*. Let us suppose that every appearance indicates the continuity of the sarcodic substance and of the nervous fibre; this appearance will become almost a certainty, if, on the course of the nervous fibre, in the neighbourhood of the chromoblast, we discover granulations of pigment which have evidently been given up by the sarcodic extensions returned on themselves. We have endeavoured to represent all the cases of this kind that we have been able to meet.

As the inquiry regards parts which had not been dried, but were observed *in situ* in the thin lamina which extends from one ray of the fin to the other, the relations observed between the parts can only be natural. If the instances are so rare, it is first because the continuity which we try to observe exists in a plane perpendicular to the visual one, and secondly because only a small number of chromoblasts show themselves in a state of isolation favourable for observation, even in the case which we bring forward; we have not thought it right to dissemble the considerations which might plead against an appearance evidently conformable to the theory, but the demonstration of which we shall not,

however, regard as rigorous, so long as it will not have been drawn from embryogenic study.

In Fig. 3 *c*, we have evidently to deal with a nervous thread, characterized by the presence of a nucleus. We must remember that there is not a trace of laminous fibres in all this tissue. This nervous fibre appeared to us to continue directly with a pale chromoblast having numerous ramifications. The union seems to be proved by some granulations scattered over the nervous thread near the chromoblast. Fig. 3 *f* shows a similar arrangement, although less clearly.

The same arrangement is seen also in Fig. 2 and Fig. 3 *b*. There, however, the filaments which appear really to continue with the chromoblast, were not, so far as they were visible, provided with nuclei. The same may be said of Fig. 3 *e*, which is certainly the most characteristic of all those that we have observed. The chromoblast was large, and very much retracted on itself like a sphere. From the edge of the chromoblast was seen to extend a thin filament, which could be traced pretty far, and which it was hard to avoid considering as a nervous thread, although it presented no characteristic nuclei. But it offered, on the other hand, certain interesting peculiarities near the outside of the chromoblast, inside which the latter's opacity of course prevented the pursuit. This thread offered in this place strongly-marked sinuosities, which are not usual on the course of the nervous fibres. But, besides, we discovered easily on those sinuosities pigmentary granulations analogous to those contained in the sarcodic substance. Error was impossible. Beyond the first sinuosity especially were seen two of those granulations, isolated and perfectly recognizable. Those sinuosities, those granulations, appeared really to indicate that the fibre there approaches its termination, enveloped in the sarcodic substance when the latter displays itself. There exists there, then, intimate contact, if not continuity of substance between the nervous fibre and the chromoblast.

One difficulty indeed appears. The influence of the nervous element on the contractile element is unquestionable, but it in no way indicates what may be the relations or the connections between the two elements. Does the nervous fibre become lost in the midst of the contractile substance on coming into contact with the nucleus of the chromoblast? This is a first hypothesis, which we confess appears to us highly probable, but which has not been demonstrated. Influence exerted at a distance from the chromoblast by adjacent nervous elements appears improbable, and besides, in opposition to what we know to exist in the other parts of the organism. There remains a third hypothesis: that the chromoblasts are merely arranged on the course of a nervous element, and in contact with it, and that only through the effect of this contact they enter into

contraction whenever the nerve is acted on. In this case there would remain to be found the termination of the nervous elements, on the course of which the chromoblasts would be thus arranged. Fig. 3 *a* appears on this subject to deserve attention. It exhibits a nervous thread which, it is true, is not characterized by the presence of a nucleus. This filament came in contact with a chromoblast, having quite a peculiar aspect. The contractile substance seemed loaded with pigmentary granulations on one side of a long and narrow nucleus, which presented all the characters of a nucleus of nervous fibre, and not at all those of the large irregular nuclei which usually accompany sarcodic bodies, and which are represented in Fig. 3 *b*, Fig. 2, Fig. 3 *d*.

Are we to suppose that the nuclei of the nervous fibres may in certain circumstances themselves become nuclei of chromoblasts, by changing their characters and undergoing a true metamorphosis. Or must we see, in the representation which we had under our eyes, only a chromoblast deprived of nucleus, and lying close to a nervous fibre at the level of one of the nuclei of the latter, giving us an instance of the simple union by contact of which we spoke.

We repeat, that these observations may require to be extended in certain directions; and although we are of opinion that we may now infer from them the reality of the connection between the nervous and the sarcodic elements, a connection conformable to theory, we cannot assert that the nature of this connection is yet completely known.

PROGRESS OF MICROSCOPICAL SCIENCE.

Rodent Cancer of the Upper Eyelid.—In describing a case of this kind to the Clinical Society (October 27), Mr. Hulke described the minute features of the stroma as follows:—It was composed chiefly of small round spherical cells like those of rete mucosum without intercellular substance, and, although differing from epithelioma, resembled it so far that he could not draw a sharp line of distinction between the two.

The Microscope in the Detection of Adulteration in Food.—A good paper on this subject appeared recently in the 'Chemical News.' It is by Mr. Walter Morris, and it deals with the subjects of coffee, cocoa, sugar, mustard, pepper, and bread. It points out the principal adulterations, and shows how they may be readily detected by the microscope. The paper is too long for an abstract.

NOTES AND MEMORANDA.

Tolles' Immersion $\frac{1}{8}$th.—In a letter to Mr. Slack, Dr. Woodward says:—"The Tolles' immersion $\frac{1}{8}$th, by which the Amphipleura pictures were made, works either dry or wet, the compensation being effected simply by altering the distance of the front lens from the other two, by means of the screw collar. There is also a low-angle extra front for ordinary work.

"I find with the high-angle fronts the following measurements:—

Angle.	Magnifies at 4 ft. focus; Micrometer screen and without eye-piece.
Dry: uncovered, 110°	225 diam.
„ for thickest cover, 140°	250 „
Immersion: uncovered, 140°	250 „
„ for thickest cover, 170° upwards ..	275 „

"With central light, and on Podura, or anatomical objects, I find this objective admirable.

"I wish I could speak as favourably of Mr. Tolles' higher powers. They are very good indeed, but I have *yet to see one of them* which will rival the so-called $\frac{1}{16}$th immersion of Powell and Lealand."

American Microscopical Apparatus.—The following brief account of the apparatus at the last American Association is given in the September No. of the 'American Naturalist':—Among the novelties may be noticed the observation of the electric induction spark by the micro-spectroscope, by Prof. Vander Weyde; the oblique illumination of transparent objects under high powers by means of light reflected from a plane mirror lying upon the stage and directly beneath the mounted object—a little expedient of great practical convenience, also by Prof. Vander Weyde; the adoption of the Wenham Binocular arrangement by Zentmayer; and the somewhat general introduction into use of the eye-piece condensers with a wide horizontal illumination (for binoculars) upon the plan proposed by Prof. Ward at the Troy meeting last summer. Mr. Bicknell places the stop-plate between the lenses of the condenser, instead of below them; and Prof. Ward, while retaining the eye-piece arrangement for use with low powers, for high powers combines the centring adjustment, Iris diaphragm, and stop-plate, with an achromatic combination of larger angle and more perfect corrections. The committee on uniform standards in the powers of objectives and eye-pieces being unprepared to report, Messrs. Ward and Bicknell reported verbally, and the committee was continued until the next meeting. While an exact uniformity in the amplifying power of lenses in the same denomination is not to be looked for, it is believed that much of the existing confusion may be remedied. Many microscopists, the speakers among the number, have long been accustomed to alter the denomination of their lenses so as to represent, as nearly as practicable, their amplifying power when in actual use; and probably the principal makers in this country will freely co-operate with micro-

scopists attaining this very desirable result. The introduction already partially accomplished, of a grading of the eye-pieces by comparison with equivalent single lenses, 2 in., 1 in., ½ in., &c., may render this part of the subject, which seemed almost unattainable, the easiest and first to be accomplished.

CORRESPONDENCE.

Grinding Diamond Points.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Mr. Wenham having been asked the best method of grinding diamond points for fine ruling, or writing on glass, kindly gave the following valuable information, with permission to publish it.

Yours faithfully,

Henry J. Slack.

"Many years ago I tried some very fine-line ruling by a very beautiful little machine, made by the celebrated Tully. At first I used fragments and splinters of diamond, but found that I could not do any good with them, after working till my patience was exhausted. No two pieces acted alike: some with the *lightest pressure that it was possible to give* tore and shattered up the surface of the glass, breaking all the lines into a mass. Other pieces that required more pressure gave irregular lines. A piece might be found that would suit for ruling lines of one degree of fineness, but would not do for others. I therefore had to give up broken splinters as impracticable, and use turned points.

"A fragment of diamond was imbedded in a short piece of copper wire, $\frac{1}{10}$ in diameter, in the way described in my paper 'On the Construction of Object-glasses.' This was chucked in the bow lathe or 'jigger,' and another splinter of diamond, similarly mounted, was held against it as a turning tool. Both were, I suppose, about equally ground away, and you could see the dust flying off; in fact, diamonds rubbed together abrade each other just like two pieces of slate pencil will do. It is *very* easy with a delicate touch at last to bring the rotating diamond to a point as fine as a needle. *This* is the right thing for glass ruling, and I have no doubt that Nobert uses the same.

"In Peter's writing machine turned points are employed, as these only will mark in every direction. At first he used to buy his turned points from the diamond workers at one guinea each, and few of them good even at that. I explained my way of turning the points, at which he succeeded at the first attempt, and ever after that made them with his own hands. He told me afterwards that what before cost him 21*s.* did not now cost him 1*s.* He did not, however, mount them quite in my way, but thus: he split the end of the wire with a fine saw, then closed the split end on the fragment of diamond with pliers, so as to nip it fast; then wound the end round with a few coils of

very fine platinum wire, and finally with the blow-pipe run the whole together with silver solder and borax. The solder insinuated itself between the coils, filled the saw kerf and ran round the diamond, uniting the whole as a solid mass. This made a very firm and substantial job of it. Of course a very clumsy piece of diamond will serve as the turning tool or rather rubber. I assure you that this is a very easy operation.

"I hope that this contains a hint that may be of service to you. You can make any use of it that you think proper. Publish it if you like, for these little dodges are too often kept as trade secrets, to the detriment of their utility.

"Yours sincerely,

"H. J. Slack, Esq." "F. H. Wenham.

Mr. Tolles and Mr. Bicknell.

To the Editor of the 'Monthly Microscopical Journal.'

November 6, 1871.

Dear Sir,—Enough has been said to convince minds conversant with optical science of the loss of aperture on objects in balsam or fluid. Mr. Tolles, however, still disbelieving, puts a case. If he is clever enough to mount a *diatom* in balsam, between two hemispherical lenses, or enclose it in the centre of a tiny spherule of hard balsam, or gum, then of course it can be illuminated from all angles, and seen by the full aperture of an object-glass (the refraction of the material will not influence the result, as the rays proceed in straight lines through the surface). But if I understand Mr. Tolles' not very perspicuous summary correctly, he brings this profound illustration forward to show that the angle of aperture is in no way diminished, when the object and front lens are both immersed in fluid. Does he really expect us to believe that he can obtain all his aperture by the back combinations alone, and that these are capable of receiving or transmitting 170°, or even anything near the least angle of 90° that he has shown? or that any of his objectives, duly adjusted for an immersed object, and thus showing a large aperture when measured in air, will retain the same angle when the front is immersed in a body of water? If so, further comment is useless, as it would no longer be a scientific discussion of any general interest, but merely an individual one—a rather hopeless attempt to convince him, by explaining primary laws of refraction, or the very a, b, c, of optics. His position is an impossible one. In a corrected high-power object-glass, when immersed, the focus does not fall in the centre of the hemisphere of the front lens, but is considerably beyond it.

In the last Journal there are some remarks by Mr. E. Bicknell. At page 226, paragraph 4, he applies the terms "deception" and "misleading people" to Messrs. Powell and Lealand, and the same to Dr. Woodward, for "knowingly" putting forth work done by a higher objective as that from a $\frac{1}{8}$th. I consider that this is hardly fair, and is uncourteous to the gentlemen named. A scientific microscopist gives the diameters with his illustrations, states the aperture, and the

nominal power of the object-glass; this quite meets the case. In such a difficult and complex arrangement as a high-power object-glass, it is almost impossible for all the makers to work to the same magnifying standard. From an early date, $\frac{1}{8}$ths were $\frac{1}{9}$ths or $\frac{1}{10}$ths, and some now approach to $\frac{1}{12}$ths in power. There is almost the same discrepancy as in the nominal and real horse-power of steam-engines, by makers who vie with each other to give the best measure, and anyone that now obtains a $\frac{1}{12}$th for what was formerly an $\frac{1}{8}$th, may congratulate himself in getting more for his money.

Yours sincerely,

F. H. Wenham.

A Mineralogical Microscope.

To the Editor of the 'Monthly Microscopical Journal.'

New University Club, *Nov.* 8, 1871.

Sir,—Dr. Lawrence Smith contrived a form of microscope for examining minerals, one of the features of which is that objects are viewed from the lower side. My own pursuits are such as to require this, but I can obtain no definite answer as to form and price of instrument from those London microscope makers whom I have consulted.

I should be very glad if your readers, both here and in America, would forward my chemico-mineralogical work by giving me any information. Of course it is an object to use such apparatus as I have, which can be adapted to the new instrument.

I would also ask whether this form of microscope might not be made to take the place of the laboratory spectroscope.

I am, Sir, your faithful servant,

Marshall Hall.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *Nov.* 1, 1871.

W. Kitchen Parker, Esq., F.R.S., F.Z.S., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks given to the respective donors.

The Secretary announced that he had received from Mr. Stanistreet, for the Society, two beautiful specimens of his engraving, or Micro-ruling, on glass. Also a photograph of his machine. There was an engraving from the photograph which was rather more intelligible

* Secretaries of Societies will greatly oblige us by writing their report legibly—especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

than the photograph in the 'English Mechanic.' Both the specimens of ruling were very beautiful, but the one on the slide was much more elaborate than that in the ornamental circular frame.

The Secretary also stated that the Council proposed, if they could obtain the use of the room in which the Society's meetings were held, to have another Scientific meeting on the Wednesday of the third week in January next, the meeting to consist of the Fellows and a few visitors. The Council hoped that every Fellow would endeavour to bring all that was new or fine in reference to recent discoveries. The precise date of the meeting would be fixed as early as possible, and then communicated to the Fellows by circular without delay. He believed it most probable that the room could be used on the day already mentioned.

Dr. Braithwaite then read a paper "On the Structure of Bog Mosses."

In answer to a question from the President, Dr. Braithwaite said that the leaves whose characters he had been describing were not all constant throughout the species, but that they do vary on the plant.

The Secretary announced that three very fine photographs of *Degeeria domestica* had been received from Dr. Woodward of the U. S. Army Medical Department, accompanied by a short descriptive paper.

Mr. Slack said he had hoped Mr. Wenham would have been present that evening, in order that he might have given an explanation of his method of illumination to which Dr. Woodward had alluded. He (Mr. Slack) believed that the action of that mode of illumination depended upon the fact that the object must rest upon the bottom of the glass slide, and that the covering glass was made to act as a mirror throwing light down upon it; consequently there would be opaque illumination just as the object was opaque. But if the object was partially transparent a portion of light would go through it. Some of the light might enter the object and reach the eye by internal reflexions, or refractions. He thought Mr. Brooke would be able to confirm this explanation.

Mr. Brooke said he believed Mr. Slack had correctly described the effects that would be produced by Mr. Wenham's mode of illumination. The explanation of it was, that the objects were partially seen by reflected light thrown down upon them from the covering glass, and they were therefore so far seen opaquely: they might also transmit certain rays that came less obliquely, and they would thus be rendered visible both by the transmitted and the reflected rays.

Mr. Slack said the photographs showed characters very near to those described by Dr. Pigott, and similar to the sketches he had published in the 'Student.'

Mr. W. Saville Kent then read a paper "On Professor James Clark's Flagellate Infusoria, with description of New Species."

Mr. Kent, replying to the inquiries of the President, said that he considered the presence of the flagellum seen in *Monas*, would place it in a higher class of Protozoa than the Rhizopoda; that some of the forms he had described had a kind of chitinous sheath, as in *Cothurnia;* that the bell-shaped "collar" shown in his (Mr. Kent's) diagrams was

an expansive membrane of extreme tenuity modified from the sarcode; and that with regard to certain individual portions of sponges bearing flagellate appendages, those organisms ought to be classed among the higher Protozoa, combining the characters of the Flagellate Infusoria and ordinary Rhizopoda, with a skeletal superstructure, and complex canal system essentially their own.

Mr. Slack said he would venture to remind the President of a specialized organ in the Amœba, first discovered by Dr. Wallich, who came ultimately to the conclusion that it was developed in a stage of the creature's existence. He (Mr. Slack) had seen this organ, and it was certainly much more specialized than a simple flagellum.

Mr. Stewart said a short time since he was examining a specimen of a calcareous sponge in the early spring months, and he observed that on withdrawing it from the water, a milky fluid escaped from it. On submitting this to the microscope he found the milkiness was really due to little masses of the amœboid particles of the sponge moving actively in the water by the aid of from one to three, or (he fancied in some cases) even four flagella attached to each particle of the compound masses. He did not notice the funnel-like membrane. He kept these particles alive some time, and noticed that the flagella were gradually absorbed, and that motion was then effected by pseudopodia the same as in Amœbæ. He thought it possible that these bodies might rather represent a form of gemma, than a mere detached group of the ordinary particles of the sponge. With regard to the connection between sponges and cœlenterates, the setting aside of certain of these particles to perform a special function for the good of the entire organism, seemed by this first indication of tissue formation to point to an affinity with some more complex animal.

Mr. Slack then reminded the Fellows that some time since their attention had been called to a creature called a Stentor, and named after the gentleman who had sent an account of it to the Society, *Stentor Barrettii*. He (Mr. S.) had then pointed out that if Mr. Barrett's statements were confirmed the creature could not possibly be a stentor. Mr. Kent had been kind enough to bring down a drawing of what he thought was the animal not quite correctly described as a stentor. It certainly seemed to be the same creature. The appearance of spines was not at all uncommon. According to Stein they belonged to the ordinary stentors, were protruded on certain occasions, and on being withdrawn left no mark behind. He thought it probable that Mr. Barrett had from some cause or another overlooked the body cilia characteristic of stentor, and had misinterpreted appearances which led him to assume that his creature possessed organs not found in any stentor. Mr. Kent submitted to the meeting a drawing of *Stentor polymorphus*, Claparède (published 1861), and in reply to Mr. Slack stated that he had recently met with the same form himself, and considered it to be identical with the species introduced to the Society last year, and figured in the Society's 'Transactions' by Mr. Barrett under the name of *S. Barrettii*. The author quoted considered it differed from previously described species in inhabiting a tube, in the possession of stiff setæ, and in having no vibratile cilia distributed over the surface of the body. The two positive characters given, Mr. Kent

affirmed, were common to the species of which he submitted a drawing, while the non-detection of vibratile cilia by Mr. Barrett he attributed to the insufficiency of magnifying power, or inadequacy of the means of illumination he had employed.

Mr. T. Charters White, in answer to a question from Mr. Slack, said he had once seen a stentor as large as that described by Mr. Barrett, but he could not say that it was identically the same; in length it occupied the whole width of the field of the microscope, when examined with a $\frac{3}{4}$rd of Smith and Beck's, and was abundantly supplied with cilia; it had a mucous agglomeration about its foot something like *Tubicularia*, into which it retreated if the stage of the microscope was struck.

The President announced that at the next meeting Dr. Carruthers would continue his description of the "Fossils of the Coal Measures," and that a communication would be read from Mr. J. Bell, "On Fermentation and its results."

The meeting was then adjourned to the 6th day of December next.

Mr. Joseph Beck stated that a note referring to him in the last number of the Journal was founded on misapprehension of what he had said.

Donations to the Library and Cabinet, from Oct. 4th to Nov. 1st, 1871:—

	From
Land and Water. Weekly	*The Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Athenæum. Weekly	*W. W. R.*
Physical Optics. By Thomas Exley, A.M., 1834	*Dr. Millar.*
Annual Report of the Commissioner of Patents for the year 1868. 4 Vols. Washington, 1869	*U. States' Patent Office.*
Journal of the Linnean Society, No. 53	*Society.*
Popular Science Review, No. 41	*Editor.*
Catalogue of Scientific Papers. Vol. V.	*Royal Society.*
Three Photographs of *Degeeria domestica*, as seen with Wenham's Black-ground Illumination. By Dr. J. J. Woodward, U. S. Army	*Author.*
Two Specimens of Micro-ruling on Glass. By John S. Stanistreet, Esq.	*Ditto.*

The following gentlemen were elected Fellows of the Society:—

Thomas Armstrong, Esq.
John Sumsion Townsend, Esq.
John Cretney Sigsworth, Esq.
Joseph Needham, Esq.
Frederick John Marriott, Esq.

WALTER W. REEVES,
Assist.-Secretary.

CORRECTIONS TO VOL. V.

Page 152, bottom line, *for* prevent, *read* present.
„ 154, line 4 from bottom, *for* Joseph Beck *read* Richard Beck.
„ 155, „ 12, *for* coin, *read* cam.
„ „ „ 14, *for* develops, *read* depresses.
„ „ „ 17, *for* coin, *read* cam.

INDEX TO VOLUME VI.

A.

Addresses wanted for the Royal Microscopical Society, 161.
Agassiz's, Professor, Future Dredging Operations, 103.
Allport, S., F.G.S., on the Microscopical Structure and Composition of a Phonolite from the Wolf Rock, 87.
Amœba, on the Conjugation of. By J. G. Tatem, Esq., 275.
Amphipleura pellucida, Note on. By Dr. J. J. Woodward, 43.
—— ——, Note on the Resolution of, by a Tolles' Immersion ⅛th. By Dr. J. J. Woodward, 150.
—— ——, "Note on the Resolution of, by a Tolles' Immersion ⅛th. By Dr. J. J. Woodward." Some Remarks on. By Edwin Bicknell, 225.
Angular Aperture, Experiments on. By R. B. Tolles, 36.
Anthophysa solitaria, laxa, and Bennettii, 264.
Apparatus, American Microscopical, 290.

B.

Barnard, F. A. P., The Examination of Nobert's Nineteenth Band, 194.
Beneden M. Edouard on a Giant Gregarina, 39.
—— on the Gregarinida and their Development, 104.
Bibliography, 60, 120, 164, 208.
Bicknell, Edwin, Some Remarks on a "Note on the Resolution of Amphipleura pellucida by a Tolles' Immersion ⅛th. By Dr. J. J. Woodward," 225.
Bicosœca lacustris and socialis, 263.
—— inclinata, 264.
Blood, on Some Improvements in the Spectrum Method of Detecting. By H. C. Sorby, F.R.S., 9.
—— of Ceylon Deer, Mæmatozoa in. By Boyd Moss, M.D., 181.
—— Globule, the Red. By Dr. Richardson, 40.
—— Corpuscle, on the Cellular Structure of the. By J. G. Richardson, M.D., 17.
—— Corpuscle. When is it in Focus? By Dr. Tyson, 42.
Blood-vessels, Passage of Corpuscles through the. By Dr. R. Norris, 238.
Bog Mosses. By Robt. Braithwaite, M.D., 1, 268.
Brachiopoda, the Position of the, 102.
Braham, Philip, on the Crystallization of Metals by Electricity under the Microscope, 277.
Braithwaite, Robt., M.D., on Bog Mosses, 1, 268.
Bridge, H. G., Mapping with the Micro-spectroscope, with the Bright-line Micrometer, 224.
Bryozoa Marine, the Retrograde Development of. By Claparède, 98.
—— Mediterranean, 161.
Butterflies and Gnats, the Scales of, 102.

C.

Calopteryx, Agrion, and Diplax, the Embryos of. By A. S. Packard, jun., 39.
Calvert, Crace-, F.R.S., Difficulty of Experiments on Spontaneous Generation, 234.
——, Experiments on Spontaneous Generation, 199.
Carter, H. J., F.R.S., on Coccoliths, 98.
Chelifer, an Incident in the Life of a. By S. J. McIntire, F.R.M.S., 209.
Chloral Hydrate, Chloroform, Prussic Acid, and other agents, Observations and Experiments with the Microscope on. By Thomas S. Ralph, M.R.C.S., 75.
Chorda dorsalis, Structure of the, 161.
Claparède on the Retrograde Development of Marine Bryozoa, 98.
Coal Plants. By John Butterworth, 49.
Coals, on the Spore-cases in. By J. W. Dawson, LL.D., 90.
Coccoliths are Plants. Mr. H. J. Carter, F.R.S., 98.
Codosiga pulcherrima, echinata, and umbellata, 262.
Corrections to Vol. V., 296.
Correspondence:—
B., 241.
Blankley, Frederick, 107.
Butterworth, John, 49.
Davis, Henry, 48.
Hall, Marshall, 293.

CORRESPONDENCE—*continued.*
HOGG, JABEZ, 162, 242.
MOUCHET, M., 108.
RICHTER, H. C., 107.
SLACK, HENRY J., 291.
SMITH, H. L., 45.
STODDER, CHARLES, 201.
TATEM, J. G., 47.
WENHAM, F. H., 291, 292.
Cotton-seeds, the Microscopic Structure of, 235.
Crinoids, a Mineral Silicate injecting Palæozoic. By Dr. STERRY HUNT, F.R.S., 99.
Crystals, Double-refracting, on Spectra formed by the Passage of Polarized Light through, seen with the Microscope. By FRANCIS DEAS, M.A., 135.
CUBITT, CHARLES, F.R.M.S., on Floscularia Cyclops, a New Species, 83.
—— on a Rare Melicertian, with Remarks on the Homological Position of this Species, and also on the previously-recorded New Species Floscularia coronetta, 165.

D.

Darwinian Question, the, a Prize Essay, 240.
—— Theory, the, applied to Plants, 101.
Darwin's Theory applied to Flowers, 240.
DAWSON, J. W., LL.D., on Spore-cases in Coals, 90.
DEAS, FRANCIS, M.A., on Spectra formed by the Passage of Polarized Light through Double-refracting Crystals seen with the Microscope, 135.
Diamond Points, on Grinding. By F. H. WENHAM, 291.
Diatoms, Another Hint on Selecting and Mounting. By Captain F. LANG, 215.
Diatomaceæ, the Structure and Nature of, 161.
Diatomaceous Earth from the Lake of Valencia, Caracas. By A. ERNST, Esq., and H. J. SLACK, 69.
Diplograpsus pristis with Reproductive Capsules. By Mr. J. HOPKINSON, 41.
Docophorus Dennyii, 8.

E.

EDWARDS, Prof. A. MEAD, on the Employment of Dammar in Microscopy, 34.
Embryology, How to Study, 241.
Eozoon, the Mineralogy of. By Prof. STERRY HUNT, 99.
Epithelium, Regeneration of the Corneal. By Dr. H. HEIBERG, 234.

F.

Facial Arches, on the Form and Use of the. By W. K. PARKER, F.R.S., 211.
FLETCHER, Dr., on Stephanurus dentatus, 103.
Floscularia Cyclops, a New Species. By CHARLES CUBITT, 83.
—— coronetta, Remarks on, by CHAS. CUBITT, F.R.M.S., 165.

G.

Generations, Infusorial, the Circuit of. By THEOD. C. HILGARD, 227, 278.
Glass Chimney, the New Metallic Covered. By FREDERICK BLANKLEY, 107.
Glycerine, How to close Cells filled with, 105.
Graafian Follicles in Man, the Anatomy of the, 236.
Gregarine, a Giant, M. VAN BENEDEN, 39.
Gregarinida, the, and their Development. By M. E. VAN BENEDEN, 104.
Gum Dammar in Microscopy, on the Employment of. By Prof. A. MEAD EDWARDS, 34.

H.

HEIBERG, Dr. H., on the Regeneration of the Corneal Epithelium, 234.
HILGARD, THEOD. C., Infusorial Circuit of Generations, 227, 278.
Histological Preparations, an Improved Method of Photographing by Sunlight. By Dr. J. J. WOODWARD, 169.
HOGG, JABEZ, on the Mycetoma or Fungus-foot of India, 61.
—— on the Fungoid Origin of Disease and Spontaneous Generation, 156.
——, Observations on Surirella gemma, 162.
——, Hon. Sec. R.M.S., on Gnats' Scales, 192.
HOPKINSON, Mr. JOHN, on a Specimen of Diplograpsus pristis with Reproductive Capsules, 41.
HUDSON, C. T., LL.D., on a New Rotifer, Pedalion mira, 121.
——, Note on Pedalion mira, 215.
HULKE, Mr., on Rodent Cancer of the Upper Eyelid, 289.
HUNT, Dr. STERRY, on a Mineral Silicate Injecting Palæozoic Crinoids, 99.
—— on the Mineralogy of Eozoon, 99.
HYATT, Professor, on the Position of the Brachiopoda, 102.

I.

Immersion Objectives, on the Angular Aperture of. By Robert B. Tolles, 214.
Inflammation and Suppuration, Researches on. By Dr. J. M. Purser, 200.
Infusoria, Notes on Prof. James Clark's Flagellate Infusoria, with Description of New Species. By W. Saville Kent, F.Z.S., 261.
Infusorial Generations, the Circuit of. By Theod. C. Hilgard, 227, 278.

J.

Johnson, Metcalfe, M.R.C.S.E., Transmutation of Form in Certain Protozoa, 184.
——, the Monad's Place in Nature, 217.

K.

Kent, W. Saville, F.Z.S., Notes on Prof. James Clark's Flagellate Infusoria, with Description of New Species, 261.

L.

Lang, Capt. F., Another Hint on Selecting and Mounting Diatoms, 215.
Lens, the, a Quarterly Journal of Microscopy, &c., 162.
Lepidodendron, the Structure of. By Prof. Williamson, 239.
Linear Projection and Rotifers. By H. Davis, 48.
Listing, on a New Method of Producing Stereoscopic Effects, 40.
Lowne, Mr., on Pangenesis, 101.

M.

Maddox, Dr., Remarks on some Parasites found on the Head of a Bat, 144.
Man, the Anatomy of the Graafian Follicles in, 236.
Mapping with the Micro-spectroscope, with the Bright-line Micrometer. By H. G. Bridge, 224.
Matters, Mixed Colouring, on the Examination of, with the Spectrum Microscope. By H. C. Sorby, F.R.S., 124.
McIntire, S. J., F.R.M.S., an Incident in the Life of a Chelifer, 209.
McQuillen, J. H., M.D., on Microscopical Fissures in the Masticating Surface of Molars and Bicuspids, 182.
Melicertian, a Rare; with Remarks on the Homological Position of this Form. By Charles Cubitt, F.R.M.S., 165.
Menopon ptilorhynchi, 8.
Metals, Crystallization of, by Electricity under the Microscope. By Philip Braham, 277.
Microscope, Dr. Dudgeon's, 240.
——, the, in the Detection of Adulteration of Food. By Mr. Walter Morris, 289.
——, a Mineralogical. By Marshall Hall, 293.
Microscopy Extraordinary, 43.
Micro-ruling on Glass and Steel. By John F. Stanistreet. With Illustrative Remarks. By H. J. Slack, Sec. R.M.S., 151.
—— ——, an Instrument for. By J. F. Stanistreet, 274.
Micro-spectroscope, Mapping with the. By H. G. Bridge, 224.
Mitraria and Actinotrocha, 161.
Molars and Bicuspids, Microscopical Fissures in the Masticating Surface of. By J. H. McQuillen, M.D., 182.
Monad, its Place in Nature. By Metcalfe Johnson, M.R.C.S.E., 217.
Monas termo, 264.
Moss Boyd, M.D., on Hæmatozoa in Blood of Ceylon Deer, 181.
Mouchet, M., on How to Select Spectacles for Near-sight and Far-sight, 108.
Mycetoma: the Madura or Fungus-foot of India. By Jabez Hogg, 61.

N.

Nerves and Chromoblasts, on the Connection of. By M. G. Pouchet, 285.
Nirmus Nitzschii, 8.
Nobert's Plate, on the Use of the. By Dr. J. J. Woodward, U. S. Army, 26.
—— Nineteenth Band, the Examination of. By F. A. P. Barnard, 194.
—— ——. By Mr. Charles Stodder, 201.
Norris, Dr. R., on the Passage of Corpuscles through the Blood-vessels, 238.

P.

Packard, A. S., jun., on the Embryos of Calopteryx, Agrion, and Diplax, 39.
Pangenesis, Mr. Lowne on, 101.
Papers, Recent Foreign, 200.
Parasites, on some New. By T. Graham Ponton, F.Z.S., 8.
—— found on the Head of a Bat, Remarks on. By R. L. Maddox, M.D., 144.

PARKER, W. K., F.R.S., on the Form and Use of the Facial Arches, 211.
Pathology of Contagion, Dr. Sanderson's Paper on the. By JABEZ HOGG, 242.
Pedalion mira. By C. T. HUDSON, LL.D., 121.
—— ——, Note on. By C. T. HUDSON, LL.D., 215.
PHILLIPS, Mr. J. A., a Chemical Analysis of a Phonolite from the Wolf Rock, 89.
Phonolite from the Wolf Rock, on the Microscopical Structure and Composition of a. By S. ALLPORT; with a Chemical Analysis, by Mr. J. A. PHILLIPS, 87.
Pinnulariæ, the Silicious Deposit in. By H. J. SLACK, 71.
Plant-organs, on the Development of, 161.
Plants, the Darwinian Theory applied to, 101.
Podura Scales, the Structure of. By F. H. WENHAM, 6.
PONTON, T. GRAHAM, F.Z.S., on some New Parasites, 8.
POUCHET, M. GEORGES, on the Connection of Nerves and Chromoblasts, 285.
Prism, the French Erecting, a Camera Lucida. By J. G. TATEM, 47.
PROCEEDINGS OF SOCIETIES:—
Brighton and Sussex Natural History Society, 56, 111, 204, 251.
Bristol Microscopical Society, 112.
Croydon Microscopical Club, 52.
Hull Scientific Association, 260.
Illinois State Microscopical Society, 59.
Liverpool Microscopical Society, 114.
Philadelphia Academy of Natural Science, 163.
Reading Microscopical Society, 112.
Royal Microscopical Society, 50, 204, 243, 293.
South London Microscopical and Natural History Club, 115, 206, 256.
Tunbridge Wells Microscopical Society, 60.
Protozoa, Transmutation of Form in. By METCALFE JOHNSON, M.R.C.S.E., 184.
PURSER, Dr. J. M., Researches on Inflammation and Suppuration, 200.

R.

Radiolaria, Development of the, 161.
RALPH, THOMAS S., M.R.C.S., Observations and Experiments with the Microscope on the Chemical Effects of Chloral Hydrate, Chloroform, Prussic Acid, and other Agents on the Blood, 75.
Retina, the Anatomy of the, 160.
RICHARDSON, J. G., M.D., on the Cellular Structure of the Red Blood Corpuscle, 17.
——, Dr., on the Red Blood Globule, 40.
RICHTER, H. C., on some New Parasites, 107.
Rodent Cancer of the Upper Eyelid. By Mr. HULKE, 289.
Rotifer, a New. By C. T. HUDSON, LL.D., 121.

S.

Salpingœca gracilis and amphoridium, 263.
Scales of Butterflies and Gnats, 102.
—— of Gnats. By JABEZ HOGG, 192.
Scorpions, the Embryology of, 160.
SLACK, H. J., on some Diatomaceous Earth from the Lake of Valencia, Caracas, 69.
—— on the Silicious Deposit in Pinnulariæ, 71.
——, Illustrative Remarks on Mr. Stanistreet's Micro-ruling on Glass and Steel, 151.
SORBY, H. C., on some Improvements in the Spectrum Method of Detecting Blood, 9.
—— on the Examination of Mixed Colouring Matters with the Spectrum Microscope, 124.
Spectacles, How to Select for Near-sight and Far-sight. By M. MOUCHET, 108.
Spectroscope in Microscopy, 160.
Spongiadæ, the Discovery of the Animal of the, confirmed, 235.
Spontaneous Generation, the Fungoid Origin of Disease and. By JABEZ HOGG, Sec. R.M.S., 156.
—— ——, Experiments on. By CRACE-CALVERT, F.R.S., 199.
—— ——, Difficulty of Experiments on. By CRACE-CALVERT, F.R.S., 234.
STANISTREET, JOHN F., Micro-ruling on Glass and Steel, 151.
——, an Instrument for Micro-ruling on Glass and Steel, 274.
Stephanurus dentatus, or Sclerostoma pinguicola, the Structure of. By Dr. FLETCHER, 103.
Stereoscopic Effect, a New Method of Producing. By LISTING, 40.
Stigmaria, the Structure of. By Prof. WILLIAMSON, F.R.S., 237.